DOCUMENT RESUME

ED 363 774 CE 065 076

AUTHOR	Cave, George; And Others
TITLE	JOBSTART. Final Report on a Program for School Dropouts.
INSTITUTION	Manpower Demonstration Research Corp., New York, N.Y.
PUB DATE	Oct 93
NOTE	424p.
PUB TYPE	Reports - Research/Technical (143)
EDRS PRICE	MF01/PC17 Plus Postage.
DESCRIPTORS	Basic Skills; Career Education; *Cost Effectiveness; Demonstration Programs; *Dropout Programs; Dropouts; *Economically Disadvantaged; Educational Attainment; High School Equivalency Programs; Job Placement; *Job Training; *Program Effectiveness; Salary Wage Differentials; *Wages; Welfare Recipients
IDENTIFIERS	*JOBSTART

ABSTRACT

The JOBSTART demonstration program provided education and vocational training, support services, and job placement assistance to educationally disadvantaged dropouts aged 17-21 at 13 sites. An evaluation assessed whether helping disadvantaged dropouts increase their educational attainment led to increased earnings. A total of 2,312 people were randomly assigned to an experimental group that received JOBSTART services and a control group that did not. Data from 1,941 youth for whom 48 months of follow-up data were available were analyzed. Findings indicated that JOBSTART led to a significant increase in the rate at which the youths passed the General Educational Development examination or completed high school. Youths in the experimental group earned less on average than those in the control group during the first year of follow-up. In the final 2 years, experimentals' earnings appeared to overtake those of controls for the full sample. Encouraging earnings impacts included those for young men who had been arrested between age 16 and program entry and for young men and women who had dropped out of school because they had educational difficulties. Earnings impacts were very large for one site: Center for Employment Training, San Jose, California. Overall, JOBSTART led to little change in youths' receipt of public assistance. From the perspectives of taxpayers and society as a whole, the investment in JOBSTART services was not repaid through increases in earnings or other quantified benefits by the end of the follow-up period. (Appendixes include descriptions of data sources, methodological issues of the JOBSTART impact analysis, description of the cost estimation, data tables, and 84 references.) (YLB)

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JOBSTART

Final Report on a Program for School Dropouts

George Cave
Hans Bos
Fred Doolittle
Cyril Toussaint

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JOBSTART:
FINAL REPORT ON A PROGRAM FOR SCHOOL DROPOUTS

George Cave
Hans Bos
Fred Doolittle
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Manpower Demonstration
Research Corporation

October 1993

Funders of the JOBSTART Demonstration:

U.S. Department of Labor
The Rockefeller Foundation
Ford Foundation
Charles Stewart Mott Foundation
The William and Flora Hewlett Foundation
National Commission for Employment Policy
AT&T Foundation
Exxon Corporation
ARCO Foundation
Aetna Foundation, Inc.
The Chase Manhattan Bank, N.A.
Stuart Foundations

Dissemination of MDRC reports is also supported by our Public Policy Outreach funders: the Ford Foundation, The Ambrose Monell Foundation, Alcoa Foundation, and Exxon Corporation.

The findings and conclusions stated in this report do not necessarily represent the official positions or policies of the funders.

Library of Congress Cataloging-in-Publication Data

JOBSTART: final report on a program for school dropouts/
George Cave . . . [et al.].
Includes bibliographical references.
1. High school dropouts – Employment – United States.
2. Occupational training – United States. I. Cave, George.
HD6273.J63 1993
331.3'42592'0973–dc20 93-9079
CIP

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ACKNOWLEDGMENTS

Any large project such as the JOBSTART Demonstration is the work of many people. Our first debt, as was the case throughout the demonstration, is to the staff and young people at the 13 sites, whose efforts turned JOBSTART into a reality.

At MDRC, many people – most of whom were involved in the demonstration since its beginning – played a vital role in this report. Robert Ivry helped conceive the program model, raised funds for the demonstration, and reviewed drafts of all the project's reports. Gordon Berlin, Barbara Goldman, Judith Gueron, Milton Little, and James Riccio reviewed and provided comments on earlier drafts of this report.

Data processing involved the collaboration of many people. In the Information Services Department, Karen Paget coordinated data collection and systems development and supervised the survey effort (conducted by Abt Associates Inc.), and Anita Kraus did the initial computer programming for the survey data. The data on program costs were collected by a team led by James Riccio that included Barbara Fink, Johanna Walter, and Lorien Rice. Colleen Galvin produced and helped analyze the impact estimates on which the findings of this report were based. Suzanne Wagner edited the report with the assistance of Michael Wilde; Patt Pontevolpe and Stephanie Cowell typed the numerous drafts of the text, tables, and graphs.

The JOBSTART Demonstration was guided by members of MDRC's Board of Directors and its JOBSTART Advisory Committee, chaired by Bernard Anderson. For their useful comments on earlier drafts of this report, the authors would especially like to thank Bernard Anderson, Eli Ginzberg, and Alan Kistler from our Board; Robinson Hollister and Margaret Simms, who are members of the Advisory Committee; and Orley Ashenfelter and James Heckman, who served as outside reviewers of portions of the report. We would also like to thank Phoebe Cottingham, Karen Fulbright, David Lah, and Julia Lopez who – as representatives of several of JOBSTART's funders – attended an early briefing on the evaluation's findings and provided guidance on the report.

The Authors

PREFACE

When the JOBSTART Demonstration began nearly a decade ago, there was a growing realization of the employment problems ahead for young people without a high school diploma. Already, the severe decline in the inflation-adjusted earnings of school dropouts had begun, and an increasing proportion of young dropouts were outside the mainstream economy, neither working nor participating in skill-building activities. Since that time, the debate has intensified over how best to ease the transition into the workforce for those who might otherwise be unable to move readily from adolescence to eventual self-sufficiency.

JOBSTART was implemented to produce evidence of the effectiveness of one approach. That strategy consisted of working within the main federal training program for disadvantaged youths – the Job Training Partnership Act (JTPA) system – to provide a combination of basic skills education, occupational training, support services, and job placement assistance for low-skilled, young school dropouts. At the time JOBSTART began, this represented a departure from common JTPA practice, which emphasized short-term services for somewhat more employable clients. The demonstration was possible only because foundation, federal, and state and local funders were committed to experimenting with an alternative approach.

Now, as the JOBSTART Demonstration ends, interest in programs to facilitate entry into the workforce is great. The new findings in this report, based on four years of follow-up, can help guide the search for better policy. While the final results of the demonstration are less positive than had been hoped for, they do provide a basis on which to build more effective programs. JOBSTART, unlike many other youth initiatives, did lead to some long-term gains in employment and earnings, which appear to continue over time. For some subgroups and sites, the results are strongly positive. The challenge is to build on these results. We hope that the implications of the implementation and impact findings and policy suggestions explored in this report are an important beginning of that effort.

Judith M. Gueron
President

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ABBREVIATIONS

AFDC	Aid to Families with Dependent Children
BSA	Basic Skills Academy (New York, NY)
CBO	community-based organization
CCP	Comprehensive Competencies Program
CET	Center for Employment Training (San Jose, CA)
CETA	Comprehensive Employment and Training Act of 1973
CREC	Capitol Region Education Council (Hartford, CT)
DOL	U.S. Department of Labor
EGOS	Emily Griffith Opportunity School (Denver, CO)
FSA	Family Support Act of 1988
GAO	U.S. General Accounting Office
GED	General Educational Development certification (high school equivalency)
JOBS	Job Opportunities and Basic Skills Training Program
JTPA	Job Training Partnership Act of 1982
MDRC	Manpower Demonstration Research Corporation
MFSP	Minority Female Single Parent Demonstration
MIS	management information system
PIC	private industry council
SDA	service delivery area
SSI	Supplemental Security Income
STEP	Summer Training and Education Program
TABE	Test of Adult Basic Education
VICI	Ventures in Community Improvement
WIN	Work Incentive Program
YIEPP	Youth Incentive Entitlement Pilot Projects
YOU	Youth Opportunities Unlimited Demonstration

EXECUTIVE SUMMARY

This report, which completes the JOBSTART Demonstration, addresses issues closely linked to the nation's ongoing debate about how best to improve the employment and earnings prospects of low-skilled, economically disadvantaged young people, who otherwise will live outside the economic mainstream. There is compelling evidence that youths who have dropped out of high school are increasingly unable to find a job, much less a job that supports a decent standard of living. The statistics are stark: In 1992, more than half of all 16- to 24-year-olds who had dropped out of school did not work during the year. For blacks, the figures are even more discouraging, with less than 30 percent working. The results from past studies of initiatives to combat these problems have generally been negative or inconclusive. There is little solid evidence about what works.

The JOBSTART Demonstration was an unusual collaborative effort to provide such evidence. The demonstration – developed and evaluated by the Manpower Demonstration Research Corporation (MDRC) – was implemented between 1985 and 1988 in 13 sites ranging from community-based organizations to schools to Job Corps Centers. In each site, 17- to 21-year old, economically disadvantaged school dropouts with poor reading skills participated in education and vocational training, and received support services and job placement assistance. In many ways, this initiative drew on lessons from the residential Job Corps program, which provides similar – though more intensive – services and, in an influential study, was found to raise young people's earnings and to be cost-effective for taxpayers. Operating funds for the JOBSTART Demonstration came primarily from the Job Training Partnership Act of 1982 (JTPA), which supports the nation's principal employment and training program for economically disadvantaged people.

Overview of the Findings

The demonstration results presented in this report are based on a comparison of young people who were randomly assigned either to a program, or "experimental," group (given access to JOBSTART services) or to a control group (who were not). The difference (often called the "impact") that the program made over time in key outcomes such as educational attainment, employment, earnings, and welfare receipt was estimated by comparing the experiences of the experimental and control groups. The main findings, based on four years of follow-up data, include the following:

- The local sites were able to recruit the target group of seriously disadvantaged youths and implement the four core JOBSTART components: education, occupational training, support services, and job placement assistance.

- More than 90 percent of the experimental group participated in JOBSTART, and they averaged 400 hours of activities, although there was wide variation in the intensity of their participation. In addition, over the four years of follow-up, the members of both the experimental and control groups averaged 400 hours of participation in non-JOBSTART activities. The impact estimates in this report represent the incremental effect of JOBSTART services over the level of services received by controls, and although youths in the control group received a fair amount of services, those in the experimental group received substantially more.

- JOBSTART led to a significant increase in the rate at which the youths passed the GED (General Educational Development) examination or completed high school. Overall, 42 percent of those in the experimental group attained this milestone, as compared to 28.6 percent in the control group, and similar impacts were present for most key subgroups of the sample. For all groups, most of the increase in educational attainment came through receipt of a GED rather than completion of high school.

- As expected, youths in the experimental group earned less on average than those in the control group during the first year of follow-up. In the final two years of follow-up, experimentals' earnings appeared to overtake those of controls for the full sample (by approximately $400 per year), and there were similar patterns for several subgroups. In most cases, however, the magnitude of these impacts was disappointing and they were not statistically significant according to the usual tests.

- Encouraging earnings impacts include those for young men who had been arrested between age 16 and program entry ($1,129 and $1,872 in years three and four of follow-up, respectively), and for young men and women who had dropped out of school because they had educational difficulties ($726 and $592 in the last two years of follow-up).

- Earnings impacts were very large for one site in the demonstration: the Center for Employment Training (CET) in San Jose, California. Earnings impacts at CET/San Jose in the last two years of follow-up totaled more than $6,000, far larger than at any other site. When these results are combined with CET/San Jose's strong earnings impacts in the Minority Female Single Parent (MFSP) Demonstration, there is growing evidence of the strength of the program at this site.[1]

- Overall, JOBSTART led to little change in youths' receipt of public assistance, although there was a notable positive finding: Young women in the experimental group who were not mothers when they entered the program were significantly less likely than their control group counterparts to receive AFDC (Aid to Families with Dependent Children) during the later years of follow-up.[2]

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[1] See John Burghardt et al., *Evaluation of the Minority Female Single Parent Demonstration*, Vol.1, *Summary Report* (New York: The Rockefeller Foundation, 1992).

- From the perspectives of taxpayers and society as a whole, the investment in JOBSTART services – about $4,500 per experimental – was not repaid through increases in experimentals' earnings or other quantified benefits by the end of the follow-up period; the initial earnings losses were too large and the later payoffs too modest for this to occur.

The experience of JOBSTART provides a number of lessons for building future youth programs. Unlike several other youth initiatives that have been studied, JOBSTART did produce apparent impacts in the period after program participation ("apparent" because they just missed passing the usual tests for statistical significance), and these impacts do not appear to decline over time, although the trend is somewhat unclear in the last four months of follow-up. The central problems were the substantial earnings losses during program participation for some subgroups and the modest earnings gains in later years for most subgroups. The final section of this summary (and the last chapter of the full report) discusses suggestions for lessening initial losses and increasing later payoffs when operating similar programs.

The Goals of the JOBSTART Demonstration

As mentioned previously, JOBSTART was modeled after the residential Job Corps. However, the residential Job Corps cannot be offered to most young dropouts because its comprehensive services are relatively expensive, it operates in specialized centers, and only young people willing and able to live away from home can participate. JOBSTART drew on the Job Corps' experience by offering several of the same basic components in a nonresidential program. Some hallmarks of the Jobs Corps – including extensive support services and financial compensation – were not available in most demonstration sites because of severe restructions in JTPA, the primary funding source for JOBSTART programs. For the same reason, paid work experience, which is another important element in many Job Corps Centers, was not included in JOBSTART programs. Amendments to JTPA enacted in 1992 have eased this situation.

The JOBSTART Demonstration – focused on improving the lives of young, low-skilled school dropouts – sought answers to five key policy questions relating to its general programmatic approach.

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[2] More precisely, this group includes young women who either were not mothers or were not living with their children at the time they entered the program.

- ***Recruitment.*** **Could local agencies attract young, economically disadvantaged, low-skilled school dropouts into an alternative education and training program?**

Many program operators have discovered how difficult it is to reach alienated young people and provide them with the support they need to participate in an intensive and challenging program such as JOBSTART.

- ***Implementation.*** **Could sites put in place a package of services designed to address the needs of these youths while working within the constraints of JTPA funding, performance standards, and administrative practices?**

Sites participating in JOBSTART had to raise operating funds from existing public funding sources, and most relied on Title IIA of JTPA. When JOBSTART began, regulations and prevailing administrative practices encouraged local JTPA agencies to emphasize shorter-term, lower-cost programs and to enroll participants who were more employable than the JOBSTART target group. Thus, the successful implementation of JOBSTART could not be taken for granted within JTPA.

- ***Participation.*** **Would the young people respond favorably to this opportunity and make an investment of their time and effort by participating in the services?**

The conditions that make it difficult to recruit low-skilled youths into education and training programs often preclude their completing the coursework. Many are reluctant to return to a school setting and/or require extensive support services because of child care responsibilities, housing problems, or other disruptions in their lives. The financial pressures they face are severe and it is difficult to attract youths into programs that cannot pay them stipends, as is the case under JTPA rules.

- ***Impacts on educational attainment.*** **Would the program lead to an increase in educational attainment, as measured by receipt of a high school diploma or GED?**

- ***Impacts on employment, earnings, and other outcomes.*** **Would the program lead to increased employment and earnings, and impacts on other outcomes?**

JOBSTART's major goal was to increase the employment and earnings and reduce the welfare receipt of young, low-skilled school dropouts. Increased educational attainment, primarily receipt of a GED, was primarily seen as a vehicle to improve the employment prospects of youths.

The JOBSTART Demonstration provided a rigorous test of the difference that JOBSTART's combination of nonresidential services made in both the educational attainment and labor market success of young people. Youths who applied for the program were randomly assigned to a group given access to JOBSTART (the experimental group) or to a group not given that access but free to seek other services in the community (the control group). Since the two groups were created by chance, using a lottery-like process, there was only one systematic difference between them: Only those in the experimental group could receive JOBSTART services. The control group provides information on what those in the experimental group would have done if there had been no JOBSTART program: Some would have found alternative services, some would have worked, and so forth. Therefore, a comparison of the two groups' behavior over time provides an estimate of the difference that the added services the experimental group received made in their subsequent educational attainment, employment, earnings, welfare receipt, and other outcomes.

Importantly, given the diversity of the JOBSTART youths and their program experience, the demonstration's research design also provided a framework for impact estimates for subgroups of the full sample. These involve comparisons of members of the experimental group and of the control group with the same initial characteristics. Important subgroups highlighted in this summary include: men, women who were living with children of their own at entry into the program (for brevity, "custodial mothers"), all other women in the sample, men arrested between age 16 and program entry, and youths who dropped out of school for reasons related to their educational performance or experience (rather than because they wished to work or for other reasons). The gender-based subgroups are important because past research suggests that impacts might vary between women and men owing to their differences in prior work experience and child care responsibilities. Young men with an arrest record were highlighted because of the need to develop means to prevent further criminal activity among this group. Those who left school because of educational problems were analyzed separately to understand if JOBSTART could serve as an alternative to regular schools for these youths.

The Structure of the JOBSTART Demonstration

The JOBSTART Demonstration guidelines, developed by MDRC and other experts, specified the target group and the characteristics of the core service components. The local programs were to recruit 17- to 21-year-old, economically disadvantaged school dropouts who read below the eighth-grade level and were eligible for JTPA Title IIA programs or the Job Corps (which is funded under Title IVB of JTPA). The four central program components were to be implemented as follows:

- **Instruction in basic academic skills**, based on individualized curricula chosen by the sites to allow youths to proceed at their own pace toward competency goals in reading, communication, and basic computational skills.

- **Occupational skills training** provided in a classroom setting combining theory and hands-on experience to prepare participants for jobs in high-demand occupations.
- **Training-related support services** including assistance with transportation and child care, counseling, and, where possible, additional support such as work-readiness and life skills (practical, everyday knowledge) training and needs-based or incentive payments tied to program performance.
- **Job placement assistance** to help JOBSTART youths find training-related jobs.

Sites were required to offer at least 200 hours of basic education and at least 500 hours of occupational training. Some sites were able to provide all four JOBSTART components themselves, while others had to serve as brokers for the young people, helping them gain entry into services at other agencies.

Within this general framework, the 13 local JOBSTART programs (listed in Table 1) did vary, reflecting their diverse operating experiences, funding sources, clientele, and local service networks. Among the important types of local variation were: agency type (school, community-based organization, or Job Corps Center); the nature of the linkage between education and training (that is, whether they were offered sequentially or concurrently); in concurrent sites, the extent of integration of these two types of instruction; the stability of funding and program operations during the demonstration; and the strength of the implementation of the core JOBSTART components, especially training and job placement assistance.

The 2,312 youths who applied for JOBSTART and were judged eligible were randomly assigned to either the experimental or control group. Follow-up surveys attempted to reach all members of both groups 12, 24, and 48 months after they were randomly assigned. This analysis of program participation and impacts uses a sample of 1,941 youths (84 percent of all those who were randomly assigned) for whom 48 months of follow-up data were available. Impacts reported in the following sections are statistically significant (that is, unlikely to have arisen by chance) unless otherwise noted.

Participation Findings

Young people in the JOBSTART sample could have improved their job-related skills by several means: through participating in JOBSTART (for those in the experimental group) or in other education and training programs, or by learning on the job. Program impacts were measured by comparing the experiences of the experimental and control groups; in essence, the impacts are a comparison of the payoff of the investment made by experimentals to that made by controls.

The most likely means for JOBSTART to make a difference in the lives of the young people

TABLE 1

THE JOBSTART SITES

Agency Name and Location	Type of Organization	Prior Service Emphasis[a]	JOBSTART Program Structure[b]
Allentown Youth Services Consortium, Buffalo, NY[c]	Community-based	Education	Sequential/brokered
Atlanta Job Corps, Atlanta, GA	Job Corps Center	Education and training	Concurrent
Basic Skills Academy (BSA), New York, NY	Community-based	Education	Sequential/brokered
Capitol Region Education Council (CREC), Hartford, CT	Community-based	Education	Sequential/brokered
Center for Employment Training (CET), San Jose, CA	Community-based	Training with some education	Concurrent
Chicago Commons Association's Industrial and Business Training Programs, Chicago, IL	Community-based	Training	Concurrent
Connelley Skill Learning Center, Pittsburgh, PA	Adult vocational school	Education and training	Concurrent
East Los Angeles Skills Center, Monterey Park, CA	Adult vocational school	Education and training	Concurrent
El Centro Community College Job Training Center, Dallas, TX[d]	Community college	Education and training	Sequential/in-house
Emily Griffith Opportunity School (EGOS), Denver, CO	Adult vocational school	Education and training	Concurrent
Los Angeles Job Corps, Los Angeles, CA	Job Corps Center	Education and training	Sequential/in-house
Phoenix Job Corps, Phoenix, AZ	Job Corps Center	Education and training	Concurrent
SER/Jobs for Progress, Corpus Christi, TX	Community-based	Training	Concurrent

NOTES: [a] "Education" refers to basic education, often as preparation for the GED examination. "Training" refers to instruction in occupational skills needed for specific jobs.

[b] *Concurrent* programs offer basic education and occupational training concurrently from the beginning of participation. *Sequential/in-house* programs offer basic education followed by occupational training, with both components provided in-house by the agency. *Sequential/brokered* programs provide basic education and then serve as a broker for occupational training, referring participants to other agencies.

[c] In October 1990 this site was renamed The Clarkson Center, Inc.

[d] In September 1988 this site was renamed the Edmund J. Kahn Job Training Center.

was for those in the experimental group to participate in substantially more education and training activities than those in the control group, who had access to other services in the community or could be working.

- **Young people in the experimental group attended an average of 400 hours of JOBSTART activities, but behind this average there is great diversity of participation.**

These reported average total hours (which include zero hours for the 11 percent of experimentals who did not participate at all) were primarily time spent in education and occupational training (an average of 362 hours in these activities), as opposed to other activities such as life skills training. The intensity of JOBSTART varied among the sample: While 33 percent of experimentals were active for more than 500 hours, slightly more than 40 percent participated for 200 hours or less.

The average length of stay in JOBSTART was 6.8 months, with 16 percent of experimentals still active in the program 12 months after random assignment and nearly 10 percent active 15 months after random assignment. This means that for most experimentals the first year of follow-up was primarily a period of program participation and that for 16 percent the second year also included months with program activity. This duration of participation was twice JTPA's average duration of service for young dropouts during the same period and virtually the same as that of the Job Corps.

- **During the initial year of follow-up, more than three times as many experimentals as controls were active in some type of education or training. Though experimentals' participation dropped sharply in subsequent years, for the entire four-year period there remained a clear difference in participation.**

Table 2 shows experimental-control differences in the percentage of youths who ever participated in education or training; the differences are shown for the full sample and key subgroups. In the four years following random assignment, 94 percent of experimentals versus 56.1 percent of controls received some type of education or training. Experimentals averaged 800 hours in these activities (not shown in the table), whereas controls averaged 432 hours. The differences were largest during the early months of the follow-up period, when most experimentals were active in JOBSTART, and gradually disappeared by the end of the first two years. All the subgroups that were analyzed showed large differences between experimentals and controls in service receipt.

Impacts on Educational Attainment

JOBSTART succeeded in raising the rate of educational attainment.

TABLE 2

IMPACTS ON RECEIPT OF EDUCATION OR TRAINING AND ON EDUCATIONAL ATTAINMENT THROUGH YEAR FOUR FOR THE FULL SAMPLE AND KEY SUBGROUPS

Outcome and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Difference
Ever received any education or training by end of year 4				
Full sample	1,941	94.0	56.1	37.9***
Men	900	94.3	51.3	43.0***
Custodial mothers	508	95.0	59.7	35.3***
Other women	533	93.2	60.9	32.3***
Received GED or high school diploma by end of year 4				
Full sample	1,941	42.0	28.6	13.4***
Men	900	42.0	28.3	13.7***
Custodial mothers	508	42.0	26.7	15.3***
Other women	533	41.6	31.3	10.4**

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

- **JOBSTART led to a substantial increase in the rate of GED certification or receipt of a high school diploma, with most who acquired one of these educational credentials obtaining a GED in the first two years of follow-up.**

Table 2 also presents JOBSTART's impacts on educational attainment during the 48 months of follow-up, again for the full sample and key subgroups. By the end of the four years, 42 percent of experimentals had obtained a high school diploma or GED, as compared to 28.6 percent of controls, for a statistically significant difference of 13.4 percentage points. This impact was similar to the findings in an evaluation of the residential Job Corps.

- **These educational attainment impacts were large for all the subgroups studied.**

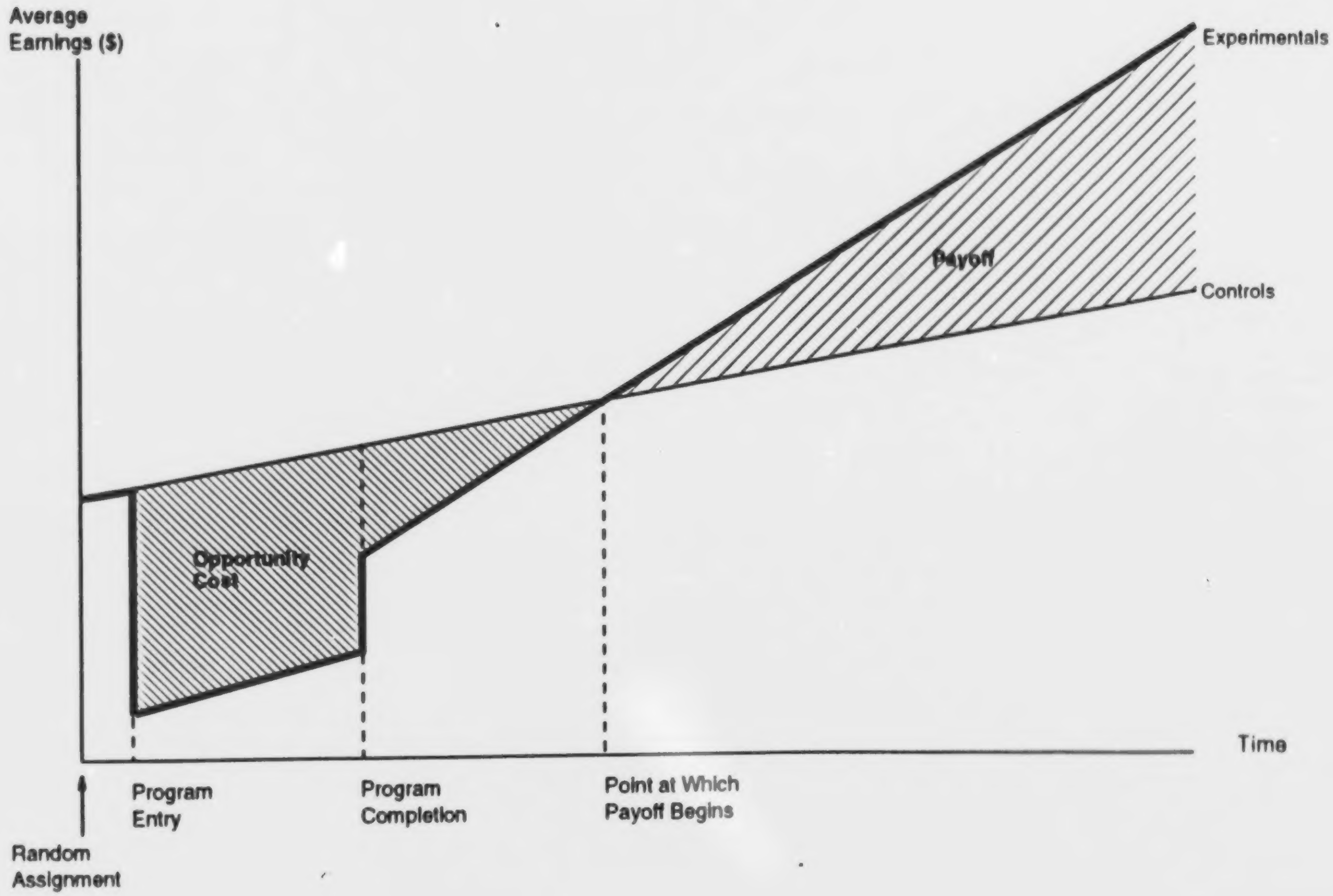
Among men, 42 percent of experimentals versus 28.3 percent of controls completed high school or passed the GED examination during the follow-up period, for an impact of 13.7 percentage points (see Table 2). Among custodial mothers, the figures were 42 percent of experimentals and 26.7 percent of controls, for an impact of 15.3 percentage points; and among all other women in the sample, the figures were 41.6 percent of experimentals and 31.3 percent of controls, for an impact of 10.4 percentage points. Numerous other subgroups defined by work experience, welfare receipt, prior education, initial reading level, reason for dropping out of school, prior criminal record, and age all showed similar large impacts.

Labor Market Impacts for the Full Sample

One of the central questions of the JOBSTART Demonstration was whether higher educational attainment would translate into greater employment and earnings for the experimental group. The theory behind JOBSTART was that the youths' initial investment in the program would in the long run lead to increases in their employment and earnings. Figure 1 shows the expected relationship between the earnings of experimentals and controls over time. During the initial period of the JOBSTART Demonstration, the earnings of experimentals – who were then active in the program – were likely to be less than those of controls. The foregone earnings owing to participation in JOBSTART are represented in Figure 1 as the shaded "opportunity cost" during the early months of follow-up. Once their participation in JOBSTART ended, experimentals could move into employment, gradually catching up with and overtaking the earnings of the control group. The earnings gains in the later months were expected to be the payoff to participants from their earlier investment in the program.

FIGURE 1

A THEORETICAL VIEW OF THE PAYOFF
OF A PERSONAL INVESTMENT IN EDUCATION AND TRAINING



- **As expected, more youths in the control group than in the experimental group worked during the first year of follow-up. In the second year, slightly more experimentals than controls worked, and in the third and fourth years the percentage working in each group was approximately the same.**

As shown in Table 3, 56.5 percent of experimentals and 60.8 percent of controls worked at some time during the first year of follow-up, for a 4.3 percentage point decrease in employment among experimentals relative to controls. In contrast, during the second year 67.5 percent of controls and 71 percent of experimentals ever worked, for a 3.5 percentage point positive impact. In years three and four, the employment rates for experimentals and controls were similar.

- **As expected, in the early part of the follow-up period experimentals earned significantly less than controls.**

As expected, experimentals earned less than controls during the first year of follow-up (see Table 3); this $499 difference was a clear opportunity cost of participating in the program. In the second year, although the proportion of experimentals working drew even with the proportion of controls, experimentals continued to lag slightly behind controls in hours worked per week and weeks worked per month. As a result, the earnings of experimentals remained slightly below those of controls during the second year; however, the difference ($121) was no longer statistically significant. The cumulative opportunity cost in the form of foregone earnings was, therefore, $620 at the midpoint of the follow-up period.

- **In the last two years of follow-up, experimentals appeared to earn more than controls, although the differences just missed being statistically significant under the usual tests.**

In the third year of follow-up, the average earnings of experimentals rose sharply to exceed those of controls by $423, an impact very close to statistical significance under the usual tests. This earnings gain persisted in the fourth year (declining slightly to $410), so that at the four-year point average total earnings for experimentals were $214 above those for controls. This four-year impact was not statistically significant, and the trend in earnings impacts is unclear in the last four months of follow-up, as shown in Figure 2. The apparent earnings gains in the last two years of follow-up are the result of small positive impacts on hours worked and wages (not shown in Table 3).

Labor Market Impacts for Men, Women, and Other Key Subgroups

Many past studies of nonresidential education and training programs have found starkly different results for men and women. Thus, it is important to move behind the findings for the full

TABLE 3

IMPACTS ON EMPLOYMENT, HOURS OF WORK, AND EARNINGS THROUGH YEAR FOUR FOR THE FULL SAMPLE

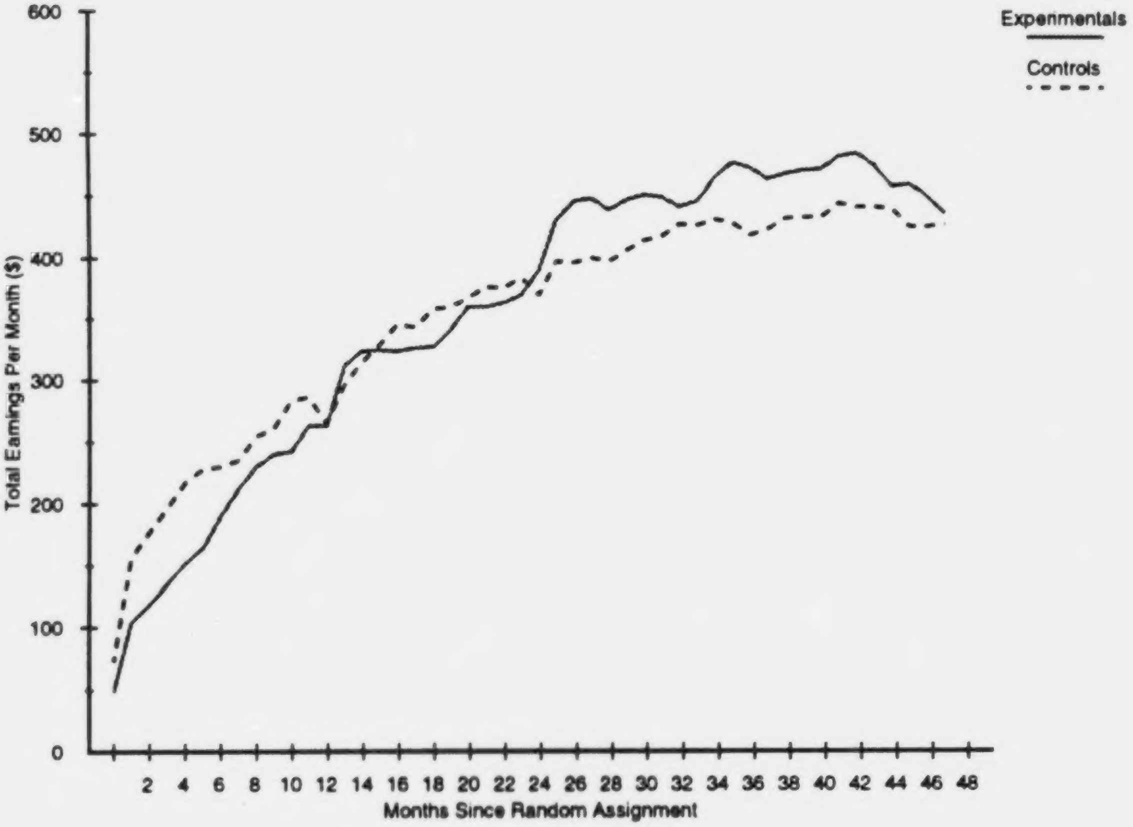
Outcome and Follow-Up Period	Experimentals	Controls	Difference
Ever employed (%)			
Years 1-4	86.4	86.0	0.4
Year 1	56.5	60.8	-4.3**
Year 2	71.0	67.5	3.5*
Year 3	61.8	61.5	0.3
Year 4	65.7	64.5	1.3
Total hours worked			
Years 1-4	3,031	3,071	-40
Year 1	441	550	-109***
Year 2	760	775	-15
Year 3	899	855	44
Year 4	930	890	40
Total earnings ($)			
Years 1-4	17,010	16,796	214
Year 1	2,097	2,596	-499***
Year 2	3,991	4,112	-121
Year 3	5,329	4,906	423
Year 4	5,592	5,182	410
Sample size	988	953	

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

FIGURE 2

MONTHLY EARNINGS FOR THE FULL SAMPLE, BY RESEARCH STATUS



NOTE: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

sample and examine the effects of JOBSTART on specific subgroups. This report focuses on three subgroups defined by gender and child care responsibilities: men (more than 85 percent of whom were not custodial parents when they entered the program), custodial mothers, and all other women in the sample. It also highlights impacts for men arrested between age 16 and program entry and for youths (both male and female) who dropped out of school for school-related reasons. In these subgroup analyses, sample sizes were of course smaller than for the full sample; therefore, statistically significant findings were less likely. Reflecting this limitation, most of the subgroup impacts reported in this and succeeding sections are not statistically significant using standard tests and should be viewed as suggestive but not definitive evidence. Despite this qualification, it is important to explore these findings because there is great policy interest in certain subgroups and because JOBSTART did result in positive – and significant – impacts on some outcomes for some groups of young people.

- **For both groups of women, after an initial period of small earnings losses, impacts in the last two years of follow-up appeared to be slightly positive.**

Table 4 shows employment and earnings impacts for the three gender-based subgroups. For custodial mothers (the top panel), the employment rate of experimentals in the first year of follow-up was slightly above that of controls, although controls earned slightly more. In the second year, a significantly higher percentage of experimentals worked, but their earnings were only slightly higher than those of controls.

In the remaining two years, the employment rates of experimentals and controls were approximately equal and experimentals again appeared to earn more ($328 more in year three and $290 in year four). For the entire four-year period, experimentals earned $625 more than controls.

For all other women (the middle panel), a slightly higher percentage of controls than experimentals worked at some point in the first year, while in the second year a higher percentage of experimentals worked. Reflecting this pattern, the earnings of controls exceeded those of experimentals in the first year; in the second year, experimentals' earnings drew even with controls'. In the third and fourth years of follow-up, experimentals appeared to pull ahead of controls (by $420 in year three and $461 in year four).

- **Men initially experienced large earnings losses, but in the last two years of follow-up earnings impacts appeared to be somewhat positive.**

Among men (the bottom panel of Table 4), a significantly lower percentage of experimentals than controls worked at some point in the first year, but in the remaining three years employment

TABLE 4

IMPACTS ON EMPLOYMENT AND EARNINGS THROUGH YEAR FOUR, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference
Custodial mothers			
Ever employed (%)			
Years 1-4	75.4	71.0	4.5
Year 1	41.0	38.8	2.2
Year 2	53.2	45.5	7.8*
Year 3	42.7	41.2	1.5
Year 4	49.1	49.3	-0.2
Total earnings ($)			
Years 1-4	8,959	8,334	625
Year 1	1,016	1,160	-144
Year 2	2,097	1,947	150
Year 3	2,700	2,372	328
Year 4	3,146	2,856	290
Sample size	257	251	
Other women			
Ever employed (%)			
Years 1-4	84.3	85.3	-1.0
Year 1	55.6	57.8	-2.2
Year 2	68.5	62.4	6.1
Year 3	55.4	54.5	0.8
Year 4	60.7	57.2	3.5
Total earnings ($)			
Years 1-4	13,923	13,310	613
Year 1	1,697	2,040	-343
Year 2	3,345	3,269	76
Year 3	4,309	3,889	420
Year 4	4,572	4,111	461
Sample size	283	250	
Men			
Ever employed (%)			
Years 1-4	94.1	94.5	-0.4
Year 1	65.7	74.9	-9.2***
Year 2	83.0	82.3	0.7
Year 3	76.6	76.9	-0.3
Year 4	78.6	76.7	2.0
Total earnings ($)			
Years 1-4	23,364	23,637	-273
Year 1	2,929	3,741	-812***
Year 2	5,435	5,831	-396
Year 3	7,401	6,957	444
Year 4	7,599	7,107	492
Sample size	448	452	

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

rates were nearly equal. Earnings for experimentals were significantly below those for controls in the first year (by $812), but the gap narrowed somewhat in the second year to $396. The earnings of male experimentals exceeded those of controls by $444 in the third year and $492 in the fourth year.

- **The employment experiences of the controls in these three subgroups provide much of the explanation for the pattern of impacts during the early follow-up period.**

One likely explanation for better initial earnings results for women is that it is easier to *improve* the employment and earnings of those who do not spend much time in the world of work (for example, young mothers) than of those who are already in the labor force but fail to find and keep steady, well-paying jobs (for example, poorly skilled young men). Thus, from this perspective, women have greater potential to improve their labor market outcomes than do men, and less to lose (in terms of foregone employment and earnings) by investing in education and training. And among women, those who are caring for children are likely to have the least prior employment experience and foregone earnings.

The JOBSTART sample followed this pattern, as shown in Table 4. During the first year after random assignment, 74.9 percent of male controls worked at some point, compared to 38.8 percent of custodial mothers and 57.8 percent of other women in the control group. During this period, when many experimentals were participating in the program, the impact on employment rates was 2.2 percentage points for young mothers, -2.2 percentage points for other women, and -9.2 percentage points for men. Men and women who were not living with children of their own apparently paid an opportunity cost for JOBSTART participation in terms of foregone employment, while young mothers (whose control group counterparts were much less likely to be working) did not. At the two-year point, custodial mothers in the experimental group had already virtually erased their initial earnings loss, while experimentals among the other women remained $267 behind their control group counterparts. For men, earnings impacts were still negative in the second year of follow-up.

Table 5 shows earnings impacts for two other important subgroups.

- **Men arrested between age 16 and program entry had large positive earnings impacts in the last two years of follow-up and for the entire four-year period.**

Among the small sample of men with an arrest prior to entering JOBSTART, experimentals initially experienced earnings losses, but in the second year of follow-up, impacts turned positive and continued to grow. In the third year the earnings impact was $1,129, and in the fourth it was $1,872 (or 37 percent of the control earnings base), a statistically significant impact even with the small sample. For the entire four-year follow-up period, earnings impacts totaled $2,491.

TABLE 5

IMPACTS ON EARNINGS THROUGH YEAR FOUR FOR OTHER KEY SUBGROUPS

Subgroup and Follow-Up Period	Experimentals ($)	Controls ($)	Difference ($)
Men arrested between age 16 and random assignment			
Years 1-4	22,835	20,344	2,491
Year 1	3,091	4,027	-936
Year 2	5,722	5,297	425
Year 3	7,052	5,098	1,129
Year 4	6,970	5,098	1,872**
Sample size	127	110	
Youths who left regular high school for school-related reasons			
Years 1-4	17,590	16,409	1,181
Year 1	2,179	2,491	-312
Year 2	4,287	4,112	175
Year 3	5,486	4,760	726*
Year 4	5,638	5,046	592
Sample size	489	436	

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

JOBSTART thus was an effective means to increase the legitimate earnings of young men who had already had an encounter with the criminal justice system. Since much urban crime is committed by young men who have prior arrests, finding that JOBSTART opened up new opportunities for this group is encouraging.

- **Youths who had dropped out of school for educational reasons had positive impacts on earnings in the last two years of follow-up.**

Those JOBSTART youths who left school for reasons such as poor grades, dislike of school, and discipline problems experienced a relatively small first-year earnings loss ($312), as shown in the bottom panel of Table 5. In contrast, those who left for employment-related reasons (not shown in the table) experienced a loss of $1,108. In the third and fourth years of follow-up, earnings impacts for those who had left school for school-related reasons were $726 and $592, respectively, with the third year being statistically significant. Earnings impacts for this group for the entire four-year period were $1,181, in contrast to negative earnings impacts for those leaving school for employment-related and other reasons.

This suggests that JOBSTART worked better for young people who were "pushed out" of regular school because of problems in that educational environment rather than "pulled out" by a desire to work or pressing problems outside the school setting. Apparently JOBSTART succeeded in creating an alternative educational setting that "felt different" from regular high school and could make a difference for young people who had serious problems in a traditional setting.

Impacts on Other Outcomes

A program such as JOBSTART could also have impacts on outcomes such as welfare receipt, pregnancy and birth rates, marriage rates, criminal activity, and drug use. For these outcomes, the main findings indicate an encouraging pattern of impacts for women who were not custodial mothers at entry into JOBSTART and for men who had been arrested between age 16 and entering the program.

- **For women who were not custodial mothers at entry into JOBSTART, there was a consistent pattern of reductions in AFDC receipt and payments, and many of these impacts were statistically significant.**

The bottom panel ("other women") of Table 6 shows impacts on welfare receipt and payments and on pregnancy and childbearing for this group. Only a small percentage of these women received AFDC in the first year of follow-up (12.6 percent of experimentals and 14.9 percent of controls), but

TABLE 6

IMPACTS ON AFDC, PREGNANCY, AND CHILDBIRTH THROUGH YEAR FOUR, BY PARENTAL STATUS AT THE TIME OF RANDOM ASSIGNMENT

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference
Custodial mothers			
Ever received AFDC (%)			
Years 1-4	84.8	81.6	3.2
Year 1	65.6	61.2	4.4
Year 2	75.5	74.3	1.2
Year 3	57.0	57.5	-0.5
Year 4	59.3	60.5	-1.2
Total AFDC income ($)			
Years 1-4	9,371	9,334	37
Year 1	2,167	2,072	95
Year 2	2,402	2,279	123
Year 3	2,310	2,343	-33
Year 4	2,493	2,641	-148
Ever pregnant (%)			
Years 1-4	76.1	67.5	8.6**
Ever gave birth (%)			
Years 1-4	67.8	57.9	9.9**
Other women			
Ever received AFDC (%)			
Years 1-4	38.0	45.1	-7.1
Year 1	12.6	14.9	-2.3
Year 2	22.0	29.9	-8.0**
Year 3	24.1	33.2	-9.1**
Year 4	30.7	39.3	-8.6*
Total AFDC income ($)			
Years 1-4	3,204	3,979	-775
Year 1	312	308	4
Year 2	604	795	-191
Year 3	1,001	1,311	-310*
Year 4	1,287	1,564	-277
Ever pregnant (%)			
Years 1-4	64.4	65.6	-1.2
Ever gave birth (%)			
Years 1-4	52.7	56.5	-3.9

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcome, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

the percentage receiving aid among both experimentals and controls gradually grew over time as over half became mothers. However, the increase in welfare receipt among controls was greater than among experimentals, with a growing difference in AFDC receipt rates during years two through four. This also led to a smaller average AFDC grant for experimentals compared to controls, especially in the third and fourth years. (Note that the figures in the table include zero dollars for those who did not receive AFDC.)

These findings on AFDC receipt and payments may be partly related to the findings on pregnancy and birth rates for this subgroup over the four-year follow-up period. Although the rate at which these women gave birth during the follow-up period was high (over 50 percent), the rate for experimentals was slightly lower than for controls.

- **For women who were custodial mothers when they entered JOBSTART, impacts were much less encouraging, with significantly increased childbearing (most noticeably among mothers who had been married at entry into the program) and no impacts on AFDC receipt.**

The top panel of Table 6 shows that for women who were custodial mothers at entry into JOBSTART there were no significant impacts on either the receipt of AFDC or the average amount received. Experimentals in this subgroup also had significantly higher rates of pregnancy (76.1 percent versus 67.5 percent for controls) and birth (67.8 percent versus 57.9 percent for controls) over the four-year period. Impacts on pregnancy and childbirth were particularly high for custodial mothers who had been married when they entered JOBSTART; for this small group, pregnancy and births among experimentals were more than 20 percentage points higher than among controls.

- **For men arrested between age 16 and program entry, JOBSTART led to reductions in arrests in the post-program period (years two through four) and in drug use in year four, although the small sample size means that these impacts were generally not statistically significant.**

Table 7 shows arrest and drug use rates for young men who had been arrested between age 16 and program entry. For all of the post-program outcomes, there was a consistent pattern of reductions in illegal behavior. Most notable was the significant impact on the use of drugs other than marijuana, where the level of use by experimentals was less than half that of controls. There appeared to be a small decline in arrests over the entire follow-up period for these young men, but the level of arrests during the four years among both experimentals and controls illustrates the continuing problems they had with the criminal justice system.

While most of these impacts are not statistically significant, when combined with the strong earnings impacts reported earlier they provide evidence that JOBSTART served as an opportunity

TABLE 7

IMPACTS ON ARRESTS AND DRUG USE FOR MEN ARRESTED BETWEEN AGE 16 AND RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference
Ever arrested			
Year 1	35.1	35.1	-0.1
Years 1-4	68.9	74.8	-5.8
Ever used any drug in year 4	25.4	31.0	-5.5
Ever used any drug in year 4, excluding marijuana	3.7	10.5	-6.9*
Ever used marijuana in year 4	25.3	30.2	-4.9

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcome, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

to change the lives of young men with a prior arrest, a group at great risk of falling into a pattern of behavior destructive to themselves and their communities.

- **JOBSTART led to a short-term reduction in arrests during the first year of follow-up for the full sample and some key subgroups.**

In the initial year of follow-up, when most experimentals were active in JOBSTART, 10.1 percent of all experimentals compared to 12.6 percent of all controls were arrested, for a statistically significant difference of 2.6 percentage points. A larger impact was observed for men without a prior arrest; 11.6 percent of experimentals reported an arrest versus 17.6 percent of controls, for a 6.4 percentage point difference. However, for both the full sample and men who had been arrested between age 16 and entry into JOBSTART without a prior arrest, there was only a small difference in arrests during the entire four-year period, implying that involvement in the program made a difference that did not continue once participation ended. The level of involvement in the criminal justice system for all men in the sample – even though lower than for those with a prior arrest – is striking; 47 percent of both experimentals and controls were arrested at least once during the four years of follow-up.

- **JOBSTART also led to a reduction in the use of drugs other than marijuana for the full sample during the fourth year of follow-up.**

In the four-year survey, respondents were asked whether they had used various drugs during the previous year, and experimentals reported significantly lower use of drugs other than marijuana compared to controls (4.1 percent versus 5.8 percent).

Earnings Impacts by Site

There is strong policy interest in the influence of site characteristics on impacts, but it is difficult to draw clear lessons from the JOBSTART Demonstration. This study was not designed to address rigorously how variations in the way the basic JOBSTART model was implemented or differences in local conditions may have affected impacts. Youths were randomly assigned to the experimental or control group in each of the 13 sites, rather than to different types of programs or different labor markets, which would be necessary for an experimental test of the influence of alternative approaches or local environments on program effectiveness.

Attempts to use differences in impacts across sites to understand the influence of program characteristics face serious problems. There were only 13 sites in the study; the average sample at each site was small (about 150) so that most site-level impacts were not statistically significant; and the *differences* among the sites in impacts on total four-year earnings – a key outcome – were also not statistically significant. Finally, the sites differed in many characteristics including the backgrounds of the youths in the sample, labor market conditions, the level of alternative services received by controls, and many dimensions of their JOBSTART programs, making it difficult to isolate the factors contributing to impacts.

Despite these difficulties in analyzing site-level impacts, one site did stand out from the others.

- **The Center for Employment Training (CET) in San Jose, California, had earnings impacts of more than $6,000 for the third and fourth years combined, and more than $6,700 for the entire four-year period. These impacts were statistically significant and substantially larger than those at any other site.**

Although site-level findings from JOBSTART can only be suggestive, these strong impacts at CET/San Jose, coupled with similar findings in the Minority Female Single Parent (MFSP) Demonstration, provide growing evidence of the effectiveness of CET/San Jose's program.

The reasons for these strong impacts remain uncertain because CET/San Jose is an unusual program in many respects. It is known for providing highly integrated education and training, but in JOBSTART this did not appear to be a factor necessarily leading to strong site impacts; the other JOBSTART site with integrated education and training had negative four-year earnings impacts. Other unique features of CET/San Jose's JOBSTART program included a clear organizational focus on employment as the program goal, little upfront screening of applicants, training in occupations in demand in the labor market, relatively intensive services concentrated during a short period of time, strong job placement efforts, and a high-wage labor market.

Overall, there was no clear pattern of impacts among the 13 sites that supports more general conclusions about the effect of program characteristics on site impacts. One aspect of program design that was of special interest in the study was the choice of offering education followed by occupational training (sequential programs) versus offering education and training simultaneously (concurrent programs). When the JOBSTART sites are divided into two groups based on this program feature, within each group there are sites with positive and negative earnings impacts.

Findings on the Benefits and Costs of JOBSTART

The benefit-cost analysis examines how overall program benefits compare to overall program costs from three perspectives. In doing so, the benefit-cost framework summarizes many different effects that can be attributed to JOBSTART. From the perspective of the youths given access to the program, the analysis examines whether the program produced greater benefits than costs during the four years of follow-up and, if so, when during the follow-up period such a payoff began to occur. It also summarizes the program's total net costs and benefits to program funders (labeled "taxpayers," as would be the case should JOBSTART become a large-scale program) and to society as a whole.

Although it is not possible to determine a dollar value for all the costs and benefits of JOBSTART, most key items in the analysis – such as program costs, earnings increases, and changes in public assistance receipt – are quantifiable. The analytical framework allows for a comparison of the impacts produced by the increased services received by the experimental group to the net costs of providing these services. In the analysis, benefits are not projected beyond the four-year follow-up period, but there is a discussion of the likelihood that longer follow-up would change the results.

- **The costs of providing JOBSTART services were approximately $4,500 per experimental.**

The benefit-cost analysis takes into account the costs of all program activities that were related to the operation of JOBSTART. Included were the costs of participation in the education and training components of JOBSTART, as well as the costs of services received through other programs. These JOBSTART costs were $4,548 per experimental. About 85 percent of this amount financed the education, training, and job placement activities, with the remainder used for support services.

- **The net costs of the additional services received by experimentals over controls were also approximately $4,500 per experimental.**

The program's net costs reflect the *incremental* use of education and training resources by the experimental group over the amount used by controls. In the JOBSTART Demonstration, experimentals and controls received approximately equal amounts of *non-JOBSTART* services over the four years of follow-up. Because the non-JOBSTART costs for experimentals and controls are virtually the same, they cancel each other out; therefore, the *net* costs of providing education and training services to the experimentals were simply the costs of providing JOBSTART services.

- **At the end of the four-year follow-up period, JOBSTART had begun to pay off for participants.**

JOBSTART experimentals experienced a net gain in income of $141 per person over the four-year follow-up period. This small gain is the result of substantial losses in the first two years of the study, followed by a steady payoff in the last two years. Behind these changes in overall income are effects on earnings, fringe benefits, and medical coverage, as well as generally small reductions in the receipt of various forms of public assistance.

- **Women who were custodial mothers at entry into JOBSTART experienced an estimated $1,004 increase in net income.**

In terms of their family income, women who were custodial mothers at program entry benefited most from their JOBSTART experience. For them, the program produced earnings gains *as well as* increases in public assistance payments. However, much of these increases in public assistance appears to have resulted from the increased birth rate for this subgroup, implying that the higher income for experimentals had to support somewhat larger households than for controls.

- **For other women and for men, the effect of JOBSTART on their income remained negative after four years of follow-up. Program payoffs for these two groups were insufficient to offset the opportunity cost of participation or the loss of public assistance.**

Women who were not custodial mothers at entry into the program experienced positive overall impacts on earnings, despite substantial opportunity costs in the first year of the study. However, a subsequent decline in public benefits (in part owing to a slightly reduced birth rate compared to the control group) exceeded these small earnings gains, resulting in an overall loss for these women. For men, earnings gains in the third and fourth years of follow-up could not offset the large initial losses experienced by this group.

For both of these subgroups, the overall losses occurred in spite of consistent improvement in the benefit-cost picture throughout the follow-up period. Thus, if observed trends in impacts continue, the net program impact on the income of experimentals in these groups may become positive as well.

- **For taxpayers, the resources devoted to funding JOBSTART were substantial, while tax payments by participants increased only slightly and savings in public services were modest or nonexistent.**

From the perspective of taxpayers, the key comparison is the cost of program services *versus* increased government revenue (through greater tax payments by participants who earn more) and reductions in public spending. The net costs of JOBSTART services were approximately $4,500 per experimental. Compared to these costs, the quantified benefits to taxpayers at the four-year point were relatively small. For the full sample, the small earnings impact generated only a slight increase in tax payments, and spending on key public assistance programs changed only slightly. There was a $74 increase in the four-year net present value of AFDC payments and a $28 increase in General Assistance. For Food Stamps there was a $34 reduction. The overall conclusions are similar for the three key subgroups of men, custodial mothers, and all other women, although those for the other women are somewhat less negative owing to substantial savings in AFDC payments for this subgroup.

- **For society, the resources devoted to JOBSTART exceeded the benefits produced by the program.**

The small overall gains for participants included in this limited benefit-cost analysis were not nearly large enough to offset the program's overall cost. Therefore, within the confines of this analysis, from a societal perspective the program was not cost-effective.

This assessment, however, is based on the assumption that a dollar lost to taxpayers is equally valuable to society as a dollar gained by program participants: that is, there is no public value in redistributing income for disadvantaged youths through employment-oriented programs. If policy-makers do find independent value in such an approach, this could change the conclusions of the analysis.

These results could also change in other ways. First, the earnings gains observed in years three and four of the follow-up period may grow – rather than remain steady or decline – over time. However, without longer follow-up it is impossible to know whether this will occur. Second, it is possible that in a more detailed benefit-cost analysis, the observed impacts on criminal arrests and drug use (especially for men with a prior arrest) may have substantial monetary value to taxpayers. Finally, other intangible benefits produced by the program (for example, the public benefits from a more educated citizenry) may be deemed substantial.

Implications of the JOBSTART Findings for Programs and Policy

The JOBSTART findings – coupled with those from other studies – provide mounting evidence of the challenge of serving disadvantaged out-of-school youths. While the message from recent research is far from optimistic, the conclusion that no program has enduring effects is overly pessimistic. In JOBSTART, there were apparent earnings gains in the third and fourth years of follow-up, well after the end of program services. The pattern of earnings impacts observed for JOBSTART was similar to that expected for such a program: an initial period when participants forego earnings (the "opportunity costs" of being in the program), a succeeding period when they catch up with controls' earnings, and finally a period when their earnings exceed those of controls.

The central problem in JOBSTART concerned the magnitude and duration of these negative and positive earnings impacts, not the lack of any payoff or the decay of program impacts: The initial losses for some subgroups were too large and the later payoffs too modest, at least during the four-year follow-up period for this study.

The final chapter of the full report – summarized briefly here – presents ideas based on JOBSTART and other research findings, and on operational experience, about possible ways to change youth employment programs to improve earnings impacts. The difficulty in applying the JOBSTART impact findings to program implementation and operations is that trade-offs are inevitable. For example, most ways of responding to the problem of initial earnings losses (by providing jobs or income support to the youths) cost money and – unless later impacts substantially improve – worsen the benefit-cost picture. The goal of discussing various options is to help program designers and operators fashion a combination of responses to better target program recruitment, encourage more substantial participation, lessen the initial earnings losses (the opportunity cost of participation), and increase the earnings payoff in the later years.

- **Programs such as JOBSTART are likely to have their best effect if a substantial percentage of participants have serious barriers to employment.**

The relatively high employment rates for youths with fewer barriers to employment are likely to produce large initial earnings losses for such participants that will be hard to compensate for later. JOBSTART earnings impacts were strongest for those less likely to be employed in the absence of the program: men with a prior arrest and youths who left school for educational reasons rather than to take a job. Also, earnings impacts were better in absolute terms for women than for men over the entire four years, and in terms of the percentage change in the last two years. These findings illustrate the importance of including youths with substantial employment barriers in programs such as JOBSTART.

Two cautions are necessary, however, in interpreting these findings. First, programs less intensive than JOBSTART may have a different pattern of impacts because the services provided are not able to help the youths overcome the employment problems they face. Second, the nature of the program experience can change if *all* – rather than some – participants face very serious barriers to success; there will be fewer role models and success stories to help motivate youths and provide satisfaction for staff. As the percentage of harder-to-serve clients increases, program managers should more closely monitor the achievement of intermediate and long-term milestones by participants, as well as the morale and motivation of the young people and staff.

- **Nonexperimental research using the JOBSTART sample suggests that there may be a threshold level of service receipt necessary before substantial earnings impacts emerge, so there is a case for continued efforts to increase participation hours and improve program retention.**

Given the wide range of hours of participation among experimentals in JOBSTART, questions remain about how the program "worked" (that is, what its impacts were) for those who participated intensively. To answer them involves estimating impacts for subgroups defined by *post-random* *assignment behavior*, which poses special analytical problems and makes any conclusions especially tentative. Nevertheless, a nonexperimental analysis conducted by MDRC suggests that post-program earnings impacts were negative for youths in the bottom third of JOBSTART participation hours, modest (and about equal to the experimental analysis's earnings impact estimate for the full 48-month sample) for those in the middle third, and very large for those in the top third. Although these results are not definitive, they suggest that continued efforts to improve youths' program participation are important. Analysis of this and related issues will be pursued further in a forthcoming technical paper.

Several types of operational responses to these findings are possible. First, there were some obvious problems at certain sites in the way JOBSTART was structured that inhibited participation. At sites where one agency first provided education and another agency subsequently provided training (sequential/brokered sites), most of the youths never made the transition to training, which seriously lowered participation. Efforts should be made at such sites to develop agreements with training providers to give referrals from the education agency priority for admission to training; to provide opportunities for participants to explore training options during education, or even start training prior to the completion of education; to schedule the transition from education to training to avoid time lags; to streamline the application process at the training agency; and to provide a case manager at the education agency to facilitate and monitor the transition of young people into training.

Second, as discussed below, participation could be enhanced by providing the young people with financial support (preferably through paid work experience) and by helping them address the emotional and social problems they face in their daily lives.

- **Efforts should be made to reduce the opportunity costs youths must pay to participate in programs such as JOBSTART.**

There are several ways program operators and policymakers might accomplish this objective. One approach worth further exploration is to provide participants with income by helping them secure and keep training-related part-time work (scheduling program activities to allow program participation and work to be combined). This approach, which might increase program retention, has the added benefit of giving young people an opportunity to apply what they learn in the classroom to actual work situations, thereby adding relevance and meaning to their training experience. It also offers a way for those with little exposure to the world of work to become acclimated to the work environment and learn more about the importance of punctuality, relationships with supervisors, and other basic aspects of the workplace.

It is true that past research on work experience as a sole program service for young people found little effect on long-term employment and earnings. But the experience of JOBSTART and other recent youth programs suggests that work experience, *when provided in combination with skill-building services*, has the promise to improve long-term impacts.

- **The psychological and developmental needs of many seriously disadvantaged young people must be addressed or these youths will be unable to benefit from employment and training programs.**

Many of the young people in employment and training programs have lived in relative poverty and isolation for much of their lives. They have not had the adult support necessary to reach the point in their development where they are ready for the responsibilities of adulthood. Many are not even able to take advantage of skill-building opportunities such as JOBSTART. In the demonstration, staff in many sites gradually recognized the importance of addressing these issues directly and expanded the scope of their work, moving beyond narrowly defined employment issues to help youths address problems of emotional development, personal safety, housing, health care, and interpersonal skills. Some innovative programs (such as YouthBuild and state and local youth conservation corps programs) have coupled education and training with opportunities to enhance young people's self-image and to develop interpersonal and leadership skills; these opportunities often include paid work experience providing services of value to the youths' community. However, much is yet to be learned about the best ways to address young people's psychological and developmental needs within JTPA, the nation's major employment and training program, and further experimentation and innovation are needed.

- **The JOBSTART findings and other research and operational experience reinforce the importance of strengthening the link between program training and services and the job market.**

Strengthening the link between education and training and the job market through greater employer involvement is key to the success of programs and their participants. Through such involvement, programs can determine the employment needs of the local job market and train youths to meet them. In addition, employers can provide participants with opportunities to understand more clearly how their training applies to the workplace. Early links with employers also increase the possibility that they will hire participants after the program.

To create strong program/employer relationships and to place disadvantaged youths in jobs requires experienced specialists in job placement who see serving these young people – who may remain difficult to place even after program participation – as their central mission. If placement assistance is left to larger organizations such as the state employment service, which have a broad range of clients, there is the risk that hard-to-serve youths such as those in JOBSTART will "fall through the cracks." Youth employment programs would often be more effective if they developed strong job placement services.

- **Longer-term assistance after placement in a job also appears to be a promising innovation.**

Experience in JOBSTART and other youth employment programs strongly suggests that the battle is not won when a young person is placed in a job. For many youths, there is a need for continued help to adjust to the demands of supervisors and the workplace, while for others new issues of child care or transportation emerge. Further, since the initial jobs of young school dropouts rarely pay well, there is a need for staff to help youths make a favorable transition from a first job to a better, second job, or to further training and education. With the recent amendments to JTPA authorizing services for up to a year after initial job placement, such longer-term assistance will now be easier to provide.

CHAPTER 1

THE ORIGINS AND GOALS OF THE JOBSTART DEMONSTRATION

The past two decades have been hard years for young Americans with limited job skills. The changing structure of the U.S. economy has meant declining opportunities to enter higher-paying careers in manufacturing, while at the same time well-paying jobs in the growing service sector have been demanding ever-higher education levels, and the inflation-adjusted earnings of young school dropouts have starkly declined. Although most young people between the ages of 16 and 24 were able to make the transition from adolescent to employed, self-sufficient worker, many of those without a strong education increasingly found this transition difficult.

Overwhelmingly, the problem of youth unemployment in recent years has been concentrated among high school dropouts who come from poor families. Many are members of minority groups, some of whom confront the continuing existence of job discrimination.[1] A further salient aspect of the problem is the growing proportion of jobless young men who are not in the labor force – that is, no longer looking for work. In 1970, among all young men ages 16 to 24 who had no high school credential and who were not enrolled in school, only 13 percent were defined as not in the labor force; by 1991 this proportion had risen to 25 percent. Among black males the comparable figures were 40 percent in 1970 and 55 percent in 1991.[2]

For a time there was a sense of optimism that demographic changes would counteract, at least in part, these economic trends. The relative shortage of young, entry-level workers arising because of the "baby bust" of the late 1960s and 1970s was expected to force employers to recruit and train individuals they might otherwise not find attractive, pulling young school dropouts into jobs.

Unfortunately, that did not happen. As shown in Figure 1.1, the employment rate (those with any paid work as a percentage of all people in a group) of all young school dropouts in the United States has not increased over the last two decades, and the rates for males and blacks have actually declined, with a sharp drop in the recession that began in the late 1980s. The employment rate of females has been relatively stable, but low (about 30 percent), and it also declined sharply at the end of the 1980s.

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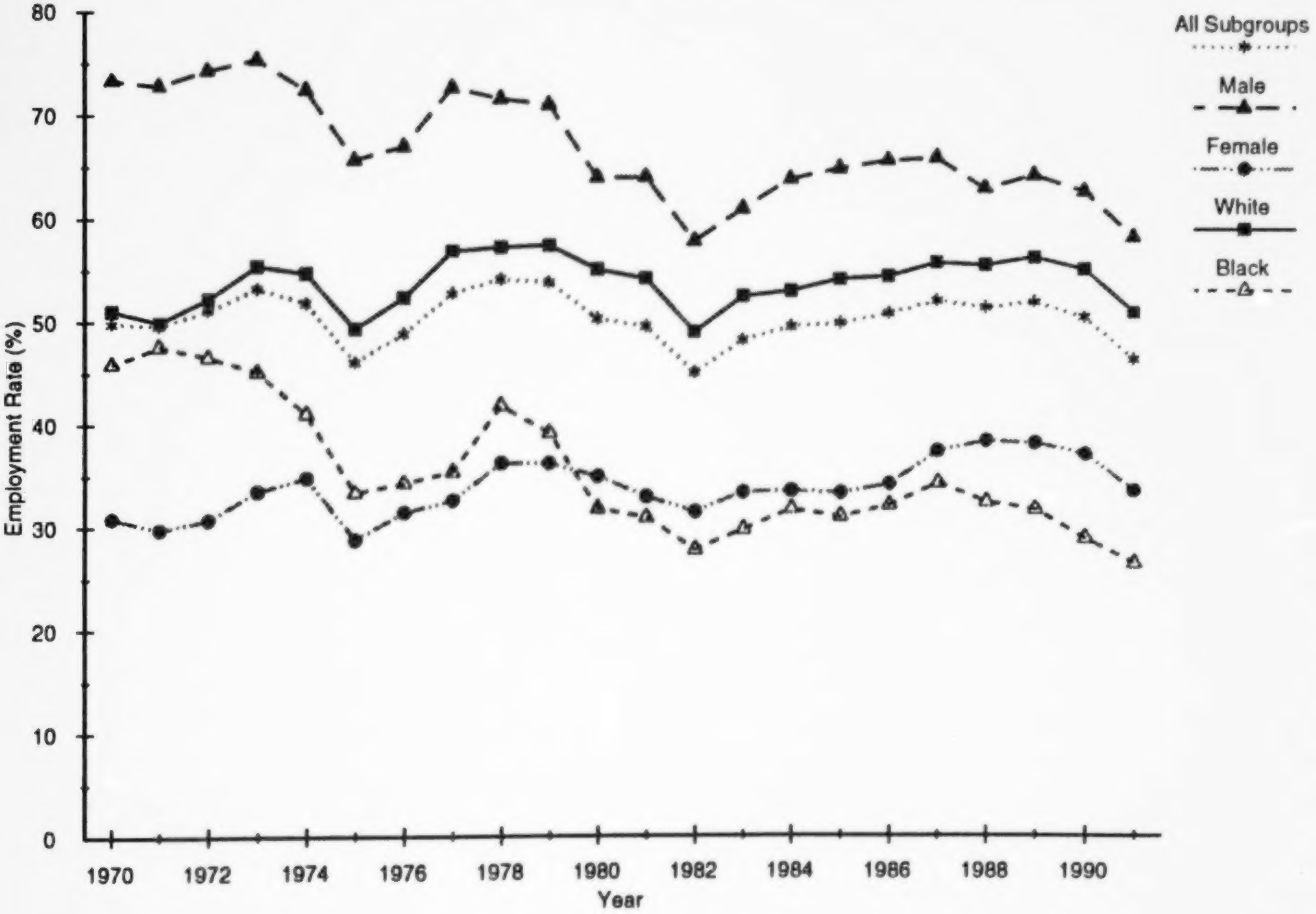
[1] See Kirschenman and Neckerman, 1991, for a recent discussion of the ways in which racial discrimination affects hiring decisions.

[2] U.S. Department of Labor, Bureau of Labor Statistics, 1992.

FIGURE 1.1

EMPLOYMENT RATES OF 16- TO 24-YEAR-OLDS WHO DO NOT HAVE A HIGH SCHOOL DIPLOMA OR GED AND WHO ARE NOT ENROLLED IN SCHOOL, BY SUBGROUP



SOURCE: U.S. Department of Labor, Bureau of Labor Statistics, 1989, 1992.

Further, among those young dropouts who worked, earnings (adjusted for inflation) dropped dramatically, by more than 40 percent between the mid-1970s and 1980s. As Freeman and Holzer have noted, "Declines in real earnings and in employment rates of the magnitudes observed are historically unprecedented. The problem facing the young less educated workers in the United States is not one of youth per se, but of change in the job market for persons of their skills."[3] As Holzer observed in another paper:

> The shift of employment away from manufacturing toward the trade and service sectors, and especially the decline of operative and laborer jobs within the manufacturing sector, . . . was particularly strong in the 1980s. These trends have eliminated many jobs in which less-skilled workers could earn fairly high wages. . . . The evidence on current employer perceptions and hiring practices, including growing employer emphasis on certain types of skills, and the current weakness in literacy levels for dropouts and minorities all suggest that any current skill imbalances will continue or worsen in the 1990s.[4]

The negative consequences of these trends extend well beyond the lives of the young people themselves, affecting both the general public and the business community. There is strong evidence that the incidence of poverty, welfare receipt, criminal activity, and unwed parenthood is significantly higher for those with poor basic skills than for the population as a whole (Berlin and Sum, 1988; Wilson, 1987). Society bears the cost in the form of social disruption and the need for increased public services.

Together these developments have prompted renewed interest in solving the problems that low-skilled young people have in making the transition to the working world. Commissions appointed by the secretaries of several federal agencies and by prominent foundations have called for renewed investment in education and training to upgrade the skills of entrants into the labor force.[5] The Family Support Act of 1988 (FSA) called for increased educational services for young women receiving Aid to Families with Dependent Children (AFDC); and the 1992 amendments to the Job Training Partnership Act (JTPA) created a separate year-round youth program, requiring that local JTPA programs target services to youths with serious barriers to employment and provide increased educational services for young school dropouts. In his election campaign, President Bill Clinton endorsed a plan to create a national network of youth opportunity centers to provide young school dropouts with an alternative way to learn the skills they need to succeed in the workplace. It is likely that legislation calling for combined education, training, and other services for low-skilled youths will soon be introduced in Congress.

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[3] Freeman and Holzer, 1991, pp. 5-6.

[4] Holzer, 1992, pp. 252-60.

[5] See, for example, Johnson and Packer, 1987; U.S. Department of Labor, Secretary's Commission on Achieving Necessary Skills, 1991; William T. Grant Commission on Work, Family and Citizenship, 1988; and National Center on Education and the Economy, 1990.

I. The JOBSTART Demonstration

The JOBSTART Demonstration tested this type of program for young school dropouts; thus, it provides evidence of the promise of the approach and of the strengths of different implementation practices, and offers lessons about the limitations of these varying practices when operated within the current programmatic context. The key features of the JOBSTART program are summarized in Table 1.1.[6] Issues of particular importance in the demonstration were the target population and the services available to participants. The 13 study sites recruited and enrolled young, primarily black and Hispanic high school dropouts who read below the eighth-grade level when they entered the program. This group had been underserved in the early years of JTPA, since many local programs enrolled people with higher reading levels who needed less intensive services. JOBSTART combined basic education, training in occupational skills, limited support services (primarily assistance with child care and transportation), and job placement assistance. Operating support consisted primarily of funds provided under JTPA, the nation's largest funder of employment and training programs for economically disadvantaged people.[7] Typically, JTPA services provided to participants – even to young school dropouts – have been less intensive than those in JOBSTART. JOBSTART was a test of what happens when JTPA is pushed to serve an educationally and economically disadvantaged group in a relatively intensive way.

Developed and overseen by the Manpower Demonstration Research Corporation (MDRC), JOBSTART was implemented in 13 study sites: four adult schools (three adult vocational schools and one community college); six community-based organizations (CBOs); and three nonresidential Job Corps programs. The demonstration ran from 1985 to 1989, before recent amendments to JTPA that make the program more conducive to initiatives such as JOBSTART.

The goals of JOBSTART were ambitious:

- The sites were to recruit young, economically disadvantaged high school dropouts with low basic skills.

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[6] JOBSTART is described in more detail later in this chapter and in Chapter 3.

[7] JOBSTART funds came from Title IIA of JTPA, the largest part of the JTPA program, or Title IVB, which funds the Job Corps Centers. Additional, special funding, provided primarily by foundations, was available to sites in the demonstration to cover a portion of the expenses of participating in the research.

TABLE 1.1

THE JOBSTART PROGRAM GUIDELINES

Target Population	To be eligible for JOBSTART, individuals had to be: • 17 to 21 years old • school dropouts without a high school diploma or GED • reading below the eighth-grade level on a standardized test[a] • economically disadvantaged[b]
Basic Education	Sites were to implement a curriculum that: • was self-paced and competency-based • was computer-managed and -assisted, if possible • was a minimum of 200 hours in length • focused on reading, communication, and basic computation skills
Occupational Skills Training	Sites were to implement a curriculum that: • was in a classroom setting • combined theory and hands-on experience • prepared enrollees for jobs in high-demand occupations • provided at least 500 hours of training • had been developed with the assistance of the private sector to ensure that graduates would meet the entry-level requirements of local employers
Training-Related Support Services	Services were to be tailored to individual needs and were to include, in addition to transportation and child care, some combination of the following: • work-readiness and life skills training • personal and vocational counseling, mentoring, tutorial assistance, and referral to external support systems • needs-based payments or incentive payments tied to length of stay, program attendance, or performance
Job Development and Placement Assistance	JOBSTART operators and/or their subcontractors were to be responsible for assisting participants in finding training-related jobs

SOURCE: Manpower Demonstration Research Corporation, 1985.

NOTES: [a] To help meet enrollment targets, each site was allowed to enroll individuals – up to 20 percent of its total JOBSTART enrollment – who read at or above the eighth-grade level.

[b] To be eligible for JTPA services – economically disadvantaged by JOBSTART standards – a person must be receiving public assistance; have family income at or below the poverty line or 70 percent of the lowest living standard income level; be homeless, under the definition of federal statutes; or, in some cases, be a handicapped adult whose own income fits within the guidelines but whose family income exceeds it.

- They were to put in place a package of services that would address the needs of the youths.

- The young recruits would have to take advantage of this opportunity and invest their time and effort in education and training activities.

- Their efforts were expected to yield them new skills, as measured, for example, by their completing high school or passing the General Educational Development (GED) test for high school equivalency certification.[8]

- Over time, those new skills were expected to translate into greater employment and earnings than the youths otherwise would have had, and into less need to rely on public assistance.[9]

Understanding whether these goals were met is the purpose of the JOBSTART evaluation.

Earlier reports on the JOBSTART Demonstration, summarized and updated in this document, found that the first four conditions for program impacts listed above were generally met: The program sites were able to recruit the target group of youths; they generally offered the intended services; many youths participated in education and training; and their participation led to increased GED receipt.[10] In sum, the JOBSTART program model received a "fair test" in the demonstration. Thus, it presents a unique opportunity to see whether and how employers responded once the disadvantaged young people had increased their educational attainment and vocational skills.

This final JOBSTART report addresses the fifth goal: whether helping young, disadvantaged school dropouts increase their educational attainment leads to increased earnings, especially in the short run. This question has not been answered by previous research. Numerous studies have found that people with higher levels of education earn more than those with no high school diploma or lower educational levels.[11] But direct rigorous tests of the impact of *increasing* the educational attainment of a group of disadvantaged young people who did not complete high school are rare.

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[8] The GED test is a national examination produced and administered by the GED Testing Service of the American Council on Education in Washington, D.C. Individual states have different criteria governing who may take the examination, different standards for passing scores, and different credentials awarded to those passing (for example, a state high school equivalency certificate or a state high school diploma). In accordance with common usage, the credential is referred to in this report as a GED certificate or, simply, a GED.

[9] The program could also affect other aspects of the young people's lives. They might be better able to live on their own instead of with their parents, more likely to postpone childbearing (because they see opportunities in the labor market), or less likely to engage in criminal behavior.

[10] See Auspos, 1987; Auspos et al., 1989; and Cave and Doolittle, 1991.

[11] For example, Berlin and Sum, 1988, found that among young men and women during the late 1970s, the payoff of an additional year of secondary school was approximately $700 in increased annual income, while a high school diploma had a "credential effect" of about $925 per year. The authors also attempted to control for the level of basic skills of individuals by including youths' scores on the Armed Forces Qualifying Test as an independent variable in a regression. The estimated effect on annual earnings of an additional grade-equivalent of basic skills (for example, progressing from a seventh- to an eighth-grade reading level) was $185.

Although, in general, increasing the skills of young people does increase their earning capacity, there are two important countervailing effects of a program such as JOBSTART, at least in the short run. Participation in an intensive program can increase the academic and occupational skills of young people. At the same time, however, it pulls participants out of the labor force. Not only do they give up earnings while in the program, but they also have less chance to gain workplace skills and seniority through on-the-job experience. For young people, especially those with low levels of educational attainment, work experience is an important source of new skills and greater job stability and wages.[12] The JOBSTART evaluation assesses which of these two effects prevails.

This report covers four years in the lives of the young people in the demonstration. While this follow-up period is long for this type of evaluation, the JOBSTART youths were at most 25 years of age when this study's follow-up period ended. Thus, this final report cannot present the long-term impacts of the program, but it is nevertheless appropriate to take stock of the program's impacts, benefits, and costs at this point. One key question addressed is whether the young people who participated in JOBSTART were better off because of the program: In the post-program follow-up period, did they reap employment gains sufficient to compensate them for the time they committed to the program and their resulting foregone earnings? A second question concerns the value of JOBSTART from a social economic-efficiency perspective. Because the program involved a substantial initial investment of funds to provide services, unless a pattern of significant, positive program impacts appears during this four-year period, it is unlikely that the social benefits resulting from the program will exceed in value the resources devoted to it. A final question is whether the experience of the JOBSTART Demonstration provides lessons for the development and implementation of new programs for low-skilled school dropouts.

All the key findings from the demonstration are presented in this report to assist the reader in understanding the JOBSTART story. The first four chapters, on the nature of the JOBSTART Demonstration and services, are modeled closely on material presented in prior reports, with findings updated to reflect both the longer follow-up data now available and the sample of individuals used for the four-year impact analysis.[13] Succeeding chapters present new, four-year findings on the employment and other effects of the program, and its benefits and costs from the perspectives of both participants and society as a whole. The analysis highlights throughout how the program worked differently for subgroups of youths that were defined based on their characteristics when entering the program: for example, gender, parental status, and ethnicity. The concluding chapter offers reflections on the lessons of the JOBSTART Demonstration for future employment and training policies and programs.

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[12] An unpublished analysis by Andrew Sum, using the *Current Population Survey*, found that the average earnings of high school dropouts increase noticeably with age during the late teenage years and early and mid-twenties. In 1986, for example, the average annual earnings of 19-year-olds were about $2,000 higher than those of 18-year-olds.

[13] As discussed in Chapter 2, JOBSTART follow-up data were collected through surveys conducted one, two, and four years after a young person became part of the research sample. Because each survey "wave"
(continued...)

Before presenting the findings, however, it is important to describe the changing research and policy context in which JOBSTART was developed, implemented, and evaluated. Key topics in this discussion are the continuing research on the effectiveness of various education and training interventions and the effect of JTPA rules on the design and operation of JOBSTART, including important legislative changes since JOBSTART began that signal greater attention to youth employment programs and more flexibility in responding to the needs of youths with severe barriers to employment. The JOBSTART Demonstration and program are then described in greater detail, and the chapter concludes with a brief overview of the JOBSTART evaluation and an outline of the remaining chapters in this report.

II. The Changing Research and Policy Context of the Demonstration

The research findings on youth employment programs available in the mid-1980s, and the then current rules of JTPA – the major funder of JOBSTART operations – strongly influenced the development of the JOBSTART program model and its implementation. More recent research findings and changes in JTPA and other programs have been important in shaping the key questions addressed in the evaluation and in developing implications for future policy initiatives.

A. Factors Influencing the Design and Implementation of JOBSTART

1. Prior research and operational experience from programs for young school dropouts. At the time the JOBSTART Demonstration began, program designers seeking insights from the previous research on programs serving young school dropouts found few solid success stories on which to base new efforts. Many types of programs had been tried, but nearly all the evaluations found unfavorable results, were inconclusive, or were seriously flawed.[14]

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[13](...continued)
succeeded in collecting information from a slightly different subset of the entire research sample, the individuals included in each report vary slightly.

[14] A common methodological problem was the absence of an appropriate group (one that was similar to participants but not served by the program) against which the experiences of the group that was served could (continued...)

The one influential exception to this pattern was the residential Job Corps, which a study found to be effective in increasing the educational attainment and earnings of young dropouts.[15] The residential Job Corps provides basic skills education, occupational training, life skills instruction, work experience, job placement assistance, health care, counseling, and other support services to youths who live at centers (often outside urban areas) and participate in the program for up to two years.[16] About 80 percent of Job Corps participants have not completed high school.

As would be expected in an intensive program of skills enhancement, Job Corps participants initially earned less than their comparison group counterparts, and this "reduction" in earnings lasted until about six months after their participation in the program ended.[17] This delay in the payoff of the program occurred because those in the comparison group had accrued more work experience (an important source of skills-building), seniority and protection against layoffs, and promotions.[18] Earnings gains began to appear about six months after participants left the program and continued throughout the remaining four years of follow-up. Not only did the research record find positive impacts for the residential Job Corps program, but it also found program benefits to be greater than program costs. Especially encouraging was the program's positive findings for young male dropouts, a group that had proven especially hard to serve in many previous programs.

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[14] (...continued)
be compared. Without such a comparison, evaluators frequently confused outcomes that followed a program with the real difference a program made – in the language of evaluation, its "impacts." For example, the *outcomes* of a program might include a post-program job placement rate of 50 percent. However, the employment rate of the appropriate comparison group might also be 50 percent, suggesting that the program had no *impact* on employment rates. See Betsey et al., 1985, and the discussion in Chapter 2 of this report for more on this issue.

[15] See Mallar et al., 1982. The results of this study are not precisely comparable to those of JOBSTART since the two studies used different research methodologies. As discussed in Chapter 2, JOBSTART used a random assignment research design, while the Job Corps study used a comparison group design. Many of the findings in the Job Corps study are reported for periods beginning with the end of program participation, while JOBSTART started tracking sample members and calculating impacts at the point of random assignment. Further, the Job Corps sample of participants excluded those who were active in the program for a short period. Since those who stay in a program for only a short time are unlikely to benefit much from participation, their exclusion from the sample probably raised the average long-term impacts presented in the analysis for Job Corps participants. In the JOBSTART study, these "short stayers" – and in fact a small group who were randomly assigned to JOBSTART but never participated – were part of the sample.

[16] Some Job Corps Centers also operate a nonresidential program. These were not included in this earlier study. As previously noted, three nonresidential Job Corps programs were included in the JOBSTART Demonstration.

[17] This would occur because of participants' foregone earnings and lost opportunities for on-the-job skills enhancement while they were in the program, as discussed earlier in this chapter.

[18] Evaluations of the Job Corps discussed the problem of the post-program transition back into the labor market that led to these initial negative impacts. See Mallar et al., 1978, 1980.

The residential Job Corps, however, could not be offered to all dropouts: It is a relatively expensive program (averaging about $5,000 per person per year in 1977, when the study was done, and about $15,000 currently), is accessible only to those willing and able to live away from home, requires development of work experience positions with employers, and is clearly not the answer for all disadvantaged youths.

Other efforts to directly connect young people with work – either by helping them look for work more effectively or providing subsidized work experience – were tested in demonstrations in the early 1980s. An evaluation of job search assistance for youths found that the program produced short-term increases in employment and earnings, but that in the long run participants were no better off than a comparison group.[19] As for the most common youth employment strategy – subsidized work experience – two evaluations failed to find any long-term impacts on educational attainment, employment, or earnings for young dropouts.[20]

Thus, the research record of the mid-1980s put the Job Corps in a special category as an effective program for raising the employment and earnings of young school dropouts.[21] Among the questions left open by the existing research, however, was whether the Job Corps approach could be successfully adapted to a new setting: a nonresidential program operated by other agencies not able to offer comprehensive support services and without the Job Corps' staff training, facilities, and curricula. A shift to a nonresidential program – as represented by the JOBSTART Demonstration – is an important one, since some of the benefits of the traditional Job Corps program seemed to result from its residential nature.[22] While the residential nature of the program may have been a factor in its success, it did pose problems for some young people. For young mothers with child care responsibilities, the program demanded too much time away from home, and it did not prove effective for them. Also, many young men and women did not wish to leave their communities.

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[19] The demonstration assessed the effectiveness of a program providing job search assistance through simulated interviews, seminars on job-seeking techniques, and help in making contact with potential employers. See Public/Private Ventures, 1983. Its finding differs from that of research on job search assistance programs for women receiving AFDC, which did find long-term employment and earnings impacts. See Gueron and Pauly, 1991.

[20] The National Supported Work Demonstration, managed by MDRC in the late 1970s, used a random assignment research design and enrolled very disadvantaged young dropouts (many with a criminal record) in a 12- to 18-month program of paid work experience with gradually increasing job responsibilities. Program impacts for this group were not positive, even though the program proved successful for long-term welfare recipients. See Maynard, 1980. The Youth Incentive Entitlement Pilot Projects (YIEPP), which offered subsidized minimum-wage jobs to high school students and dropouts who returned to school, also was ineffective for dropouts. See Gueron, 1984. While the program did increase the employment and earnings of young people still in school, evaluators found that the offer did not induce dropouts to return to and remain in regular high school. Many of those who did return dropped out a second time, and there were no effects on educational attainment, employment, or earnings for dropouts.

[21] The National Academy of Sciences, in its review of research on employment programs for young people, pointed out the distinction between the failure of research to provide adequate evidence of program effectiveness and the finding that a program is ineffective. (Betsey et al., 1985.)

In 1983, the National Academy of Sciences convened a panel of experts on youth programs. Their assessment – summing up research findings – recommended further testing of the Job Corps approach in a nonresidential setting using random assignment to produce the most reliable findings.[23] The JOBSTART Demonstration was, in part, a response to this call.[24]

In addition to this research record, program operators serving young, disadvantaged dropouts had identified a number of lessons that also informed the development of the JOBSTART Demonstration.[25] When serving these youths, who often look back on past educational experiences with dissatisfaction, programs have to actively seek out participants rather than passively wait for volunteers to come forward. Program operators have also learned that achieving continued participation is not easy: Counseling and peer support have often proved useful in improving young people's self-esteem and motivation, but even with these efforts, participation levels can be less than expected.

Finally, program operators have increasingly become sensitive to the multiple needs of clients. For some economically disadvantaged young people, a low level of basic skills prevents them from taking advantage of occupational training. In addition, as is the case for many young people, economically disadvantaged youths may not be experienced in setting goals, making plans to achieve them, and following through with effective action. And finally, many young people have a pressing need for immediate income, for themselves or their family, so programs must help them find a means of financial support while they invest in their future by enhancing their skills.

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[22] For example, in a residential program, it is much easier to provide an intensive program of support services (including counseling outside class time, positive peer support, recreational activities, and health care) than when young people are active in the program for at most eight hours a day. Furthermore, the decline in criminal activity and substance abuse observed for Job Corps participants (especially during program participation) was partly attributable to their isolation in residential centers outside urban areas, or at least outside their previous neighborhood.

[23] Random assignment is generally recognized to be a reliable method of measuring the effectiveness of new employment and training programs. As discussed later in this report, it was used in the JOBSTART Demonstration.

[24] As the later discussion of the JOBSTART program model and its implementation will make clear, JOBSTART did not offer the same comprehensive list of support services available in the Job Corps. Nor did it use the same curricula in education or training, except in the three sites that already operated a nonresidential Job Corps program. Nationally, about 10 percent of Job Corps participants are in nonresidential programs.

[25] Many of these lessons are summarized in 70001 Training and Employment Institute, 1988, and Public/Private Ventures, 1990.

2. The initial programmatic context of the JOBSTART Demonstration. Because the JOBSTART Demonstration did not have special program funding to support site operations, it was shaped in important ways by the need to find funding for local JOBSTART sites from existing programs. Since JTPA was the most likely source of local operational funds, its provisions and the local interpretation of them were central to the structure of the project.

For a decade, the Job Training Partnership Act (JTPA) of 1982 has been the federal government's major program for funding employment and training for economically disadvantaged adults and youths. JTPA distributes the majority of its funds to states which, in turn, pass along most of what they get to local administrative entities called service delivery areas (SDAs).[26] The federal JTPA statute sets general rules for program eligibility and allowable types of activities. An SDA's staff and private industry council (PIC) – often operating like a board of directors for the agency – determine what specific types of services are to be offered, which groups will get priority for services, and how service providers under contract to the SDA are to be evaluated and paid.

The manner in which JTPA was initially implemented during the mid-1980s presented operational constraints that had to be taken into account in the design and implementation of the JOBSTART Demonstration:

- **Performance standards that made SDAs hesitant to serve youths with very poor skills.** In designing and applying the performance measures used during the first five years of JTPA, federal, state, and local administrators focused on the proportion of participants placed in a job, their wages, and the cost per "success story."[27] This encouraged SDAs and service providers to choose people who were more likely to achieve these successes at relatively modest costs.[28] In seeking to serve school dropouts with poor skills in an intensive program, JOBSTART had to confront these issues.

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[26] Most JTPA funds under Title IIA, the largest part of JTPA, are distributed to states, using a formula based on the state's number of unemployed and economically disadvantaged people.

[27] From the early 1980s until program year 1987 (ending in June 1988), the performance of SDAs serving adults was judged by the following standards: the percentage of adults who found a job; the percentage of adults who were receiving welfare when they enrolled in JTPA and who found a job; the average wage at placement in a job; and the program cost per person entering employment. For youths, the standards included the percentage who found employment and the "positive termination rate," defined as entering employment or other quantifiable measures of program success. These included attainment of employment competencies recognized by local private industry councils, completion of a level of schooling, enrollment in further non-Title IIA training, enlistment in the armed forces, return to school full-time, or (for 14- and 15-year-olds) completion of specified program objectives. The youth standards included the cost per "positive termination." For each measure, the U.S. Department of Labor set national levels, which – at state option – could be adjusted to reflect the characteristics of those served and the conditions in the local labor market.

- **Severe restrictions on paid work experience.** Experience with public service employment under JTPA's predecessor, the Comprehensive Employment and Training Act (CETA), led Congress to eliminate most forms of paid work experience when enacting JTPA. Thus, it was difficult for JOBSTART to fund paid work experience, which had been an element of the residential Job Corps program; this led to JOBSTART as a test of education and classroom occupational training rather than education, training, and work experience.

- **Tight limitations on support services.** The JTPA statute limited spending on support services (such as transportation and child care assistance) and needs-based cash payments, and completely eliminated the payment of stipends to participants. This increased the difficulty of retaining youths in the program.[29]

The early experience under JTPA prompted allegations that the program was making little real difference in participants' lives because service providers tended to enroll more job-ready applicants (a practice known as "creaming"). Further, sharp declines in the unemployment rate during the 1980s, which allowed many more job-ready individuals to find work, caused a rethinking about whether JTPA should continue to emphasize quick placement of participants in a job.

Over time, Congress, the U.S. Department of Labor (DOL), and program operators have all expressed renewed interest in intensive programs of education and training targeted on more disadvantaged youths. Responding to the early pattern of program operation, DOL changed its administrative practices and regulations and encouraged greater provision of intensive services for youths, easing the implementation problems faced in the later stage of the demonstration.[30] Soon thereafter, amendments to the regulations (effective in program year 1988) encouraged states to choose as the key standard for youth programs one that includes measures of increased educational and skills competencies. This increased the opportunities to include young dropouts with poor skills in JTPA.[31]

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[28] Data from the mid-1980s illustrate the effects of these program priorities. During program years 1984 to 1986, when the JOBSTART Demonstration was beginning, young dropouts constituted only 11 percent of all Title IIA participants and 27 percent of all young participants. Among young dropouts served under Title IIA nationally in 1986, only 23 percent received basic education, a service likely to promote their long-term employability but unlikely to lead to immediate placement in a job.

[29] In addition, some types of performance contracts inhibited combined education and training, especially when the services were provided by different agencies. Many SDAs wrote contracts with service providers that linked payment to the achievement of the measures in the performance standards. This made it more difficult for those service providers that wished to serve youths with poor skills to be paid, since many youths might not reach the required level of performance, and complicated the administration of local programs where different agencies provided the education and training, since the youths might not have reached any performance benchmarks when they moved from the first agency to the second.

[30] In late 1987, DOL stated that "more emphasis must be placed on intensive investments in youth within JTPA" and recommended that "a significant portion of youths who participate . . . should receive competency-based instruction in either basic education or occupational skills" (*Federal Register*, December 16, 1987).

B. Recent Developments Affecting the Interpretation of JOBSTART Findings

Since the mid- to late 1980s, when JOBSTART began, research on the employment problems of youths and program reform have continued. Developments in both areas have heightened interest in the JOBSTART Demonstration.

1. Research findings. Since the start of the JOBSTART Demonstration, two additional studies of youth programs have produced findings. Initial, 18-month findings are now available from the National JTPA Study, an examination of program implementation and impacts in 16 local programs.[32] As shown in Table 1.2, this study found that, for young out-of-school women, program impacts on earnings were slightly negative (-$182) but not statistically significant over the 18 months after random assignment. For young men, earnings losses were substantial (-$854) over the 18-month period, though these losses were primarily concentrated among young males with an arrest prior to applying for JTPA. For all these groups, the time trend is not encouraging, though again the results for males are more troubling. For males, earnings losses were present in every quarter, whereas for females, small (but statistically insignificant) earnings gains appeared in the fourth and fifth follow-up quarters. Earnings impact estimates for a subgroup of youths recommended for classroom training in occupational skills (not shown in the table), forming the closest counterpart to the JOBSTART services examined in the study, were also calculated. For this subgroup the earnings losses for females were greater than for males (-$542 versus -$259). Though longer follow-up, available later in the project, will be the final word, these early findings on youths are sobering, especially in contrast to the positive – though modest – 18-month impacts for adult men and women in the JTPA study.

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[31] In addition, in 1989 DOL issued a request for proposals for the Youth Opportunities Unlimited (YOU) Demonstration. Sites applying to participate were required to operate one of three innovative programs: a work experience program modeled on Ventures in Community Improvement (VICI), which operated from 1978 to 1980; an alternative high school program modeled on High School Redirection in Brooklyn, New York; or a program of education and training modeled after JOBSTART. In 1990, seven sites were chosen for the three-year demonstration; some of them chose to operate a program modeled after JOBSTART. An implementation study is part of that demonstration, which is separate from the JOBSTART Demonstration reported on here.

[32] This study is being conducted by Abt Associates Inc., ICF, MDRC, New York University, and National Opinion Research Center (NORC), under funding by the U.S. Department of Labor. See Bloom et al., 1993.

TABLE 1.2

NATIONAL JTPA STUDY IMPACTS ON EARNINGS FOR FEMALE AND MALE OUT-OF-SCHOOL YOUTHS

	Female Youths			Male Youths		
Follow-Up Period	Experimentals ($)	Controls ($)	Difference ($)	Experimentals ($)	Controls ($)	Difference ($)
Quarter 1	726	775	-49	1,213	1,412	-199**
Quarter 2	887	943	-56	1,526	1,598	-72
Quarter 3	1,011	1,084	-73	1,652	1,803	-151*
Quarter 4	1,086	1,084	2	1,738	1,876	-138
Quarter 5	1,174	1,124	50	1,879	1,984	-105
Quarter 6	1,159	1,214	-55	1,874	2,063	-189**
All quarters	6,043	6,225	-182	9,882	10,736	-854*
Sample size	1,814	835		1,436	708	

SOURCE: Bloom et al., 1993.

NOTES: There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Indeed, the findings on youths fueled already active debate about reforms of JTPA-funded youth programs, and led to further calls for more intensive programs targeted on disadvantaged youths.

The findings from the Summer Training and Education Program (STEP) Demonstration have also contributed to the debate about youth programs, although they deal with young people still in school. This demonstration, organized by Public/Private Ventures, provided education and part-time employment to "at-risk" JTPA-eligible students in the summers after their sophomore and junior years of high school, as well as additional services during the school year. Despite early signs of positive in-program impacts on basic skills and knowledge of sexual risks and contraceptive techniques, the program had no significant longer-term post-program impacts on key outcomes such as educational attainment, employment, earnings, parenting, and welfare receipt.[33] This, plus the JTPA findings, posed starkly the difficulty of affecting the long-term prospects of economically and educationally disadvantaged young people.

A second line of recent research is relevant for the debate about education and training programs for young dropouts. Despite the general conclusion that further education increases earnings, little is known about how employers assess receipt of a GED, the primary education outcome in "second chance" programs such as JOBSTART. The assumption behind such programs is that employers will view a GED as evidence of increased skills, but little is known about whether this is the case, how long it takes for a GED to pay off, and how any such payoff might vary among subgroups of youths and types of occupations. Alternatively, and less optimistically, a GED may do little to counter the negative impression created by the fact that the young person did not finish high school. Some recent research suggests that this is a real possibility; one study comparing the earnings of school dropouts, GED recipients, and high school graduates found little difference between dropouts and GED recipients, and a clear difference between both those groups and high school graduates.[34] These findings would be consistent with the view that employers do not consider receipt of a GED as signifying higher skills levels than those of a typical dropout. Alternately, it could mean that employers rely on a high school diploma as a sign of persistence (rather than a required level of basic skills), and that receipt of a GED could not overcome the negative signal conveyed by dropping out of school.[35]

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[33] See Grossman and Sipe, 1992.

[34] See Cameron and Heckman, 1991.

[35] See Layard and Psacharopoulos, 1974.

Intense research effort has focused on this issue in recent months, and JOBSTART – even though it is a program providing more than just assistance in passing the GED – has much to contribute to the debate. Chapters 4 through 6 show that JOBSTART succeeded in increasing the percentage of youths who passed the GED and examine the payoff in the labor market.

2. **Programmatic changes**. Continued debate over JTPA led to the passage of amendments in late 1992, which become effective in July 1993. These amendments created a separate year-round youth title (Title IIC), with a focus on improving the long-term prospects of young people, and requires that 65 percent of youths served in local JTPA programs must meet one of several specified barriers to employment: basic-skills deficient, pregnant or a parent, experiencing a disability, homeless or a runaway, or a legal offender. Fifty percent of young people served must be out of school, and school dropouts under the age of 18 must enroll in an educational program. The amendments eased restrictions on the use of paid work experience as a service, reopening the program option (education and work experience) foreclosed by the original JTPA rules. They also revised performance standards to abolish cost standards and to reward services provided to "hard-to-serve" individuals, a group likely to need more intensive services such as those provided in JOBSTART. Finally, they imposed restrictions on the use of performance-based contracts, a form of service procurement that had been alleged to inhibit providing services to youths facing serious barriers to employment. While this increased interest in hard-to-serve youths came too late to affect the implementation of the JOBSTART Demonstration, it has heightened the importance of the project as an early test of a new direction for JTPA and has provided new flexibility in the design and implementation of programs for youths.

Other policy developments also signal a growing emphasis on programs of education and employment services for low-skilled youths. The Job Opportunities and Basic Skills Training (JOBS) title of the Family Support Act of 1988 expands the obligations of AFDC mothers – and especially young mothers – to participate in activities intended to increase their employability, and of states and counties administering the AFDC program to offer more education and training than typically offered under the predecessor Work Incentive (WIN) Program.[36] Expanded youth community service programs linked to education and training are also possible under other passed and proposed legislation.[37] Finally, many proposals to ease the transition from school to work for youths not attending college have been presented.[38]

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[36] Especially relevant to JOBSTART is the fact that the JOBS legislation allows states to impose a participation obligation on AFDC parents under 20 years of age who lack a high school diploma or GED regardless of the age of their child. While single parents with children under age three are normally exempt from participation in JOBS programs, it is not the case for custodial parents under age 20 who have not graduated from high school or received a GED. For this group, education is normally presumed to be the appropriate first activity in JOBS.

III. The JOBSTART Sites and the Program Guidelines

MDRC began the JOBSTART Demonstration in 1985 with two purposes: (1) to determine the operational feasibility within JTPA of an intensive program incorporating several of the key elements of the residential Job Corps, and (2) to rigorously test its effectiveness.[39] Local and state JTPA agencies provided most of the operational funding for the JOBSTART sites, but the MDRC evaluation was funded by an unusual consortium consisting of the U.S. Department of Labor, The Rockefeller Foundation, the Ford Foundation, Charles Stewart Mott Foundation, The William and Flora Hewlett Foundation, National Commission for Employment Policy, AT&T Foundation, Exxon Corporation, ARCO Foundation, Aetna Foundation, The Chase Manhattan Bank, and Stuart Foundations. Funding from this consortium also enabled MDRC to award a modest $25,000 grant to each site.

As discussed earlier, the funding structure shaped the character of the demonstration at the local level in two important ways. First, the JOBSTART program operated within existing agencies and programs under the rules and performance standards of Title IIA of JTPA or, for the nonresidential Job Corps Centers, under Title IVB of JTPA. It proved a serious challenge for the non-Job Corps sites simultaneously to follow the demonstration guidelines, the rules of Title IIA of JTPA, and the provisions in their contracts with SDAs.

Second, without special funding, sites could not be expected to make major changes in their existing programs, limiting the extent to which the JOBSTART curriculum and instructional methods could be standardized. Consequently, MDRC gave sites general guidelines for program operation specifying the type and duration of required components of the program (education, occupational training, job placement, and support services). Even within this flexible framework, however, some program operators faced major implementation challenges. For example, some sites normally offered only basic skills education or vocational training; the demonstration called for both, requiring them either to add a whole new kind of activity or to link up with other local agencies providing it. Some sites also had to adapt to a younger and less skilled student body than they normally served. The lack of special program funding also limited the extent to which non-Job Corps sites could offer the array of support services that were a part of the Job Corps program.

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[37] The Defense Authorization Bill of 1992, for example, contains the Civil Community Corps Demonstration Program for youths, which includes summer and year-round programs in both residential and nonresidential settings.

[38] For example, numerous proposals have been offered to enhance the apprenticeship programs in the United States to help more young people find stable employment.

[39] See Auspos, 1987, for a discussion of the origins of JOBSTART and its early implementation.

The demonstration was thus a hybrid: part evaluation of existing programs and part test of a new program. The basic program differed from site to site in myriad details, but the variety did permit a test of how a scaled-down Job Corps-type program could operate under existing rules in different kinds of established agencies. If the demonstration showed positive results, it would be easier to replicate the program widely.

A. The Sites

MDRC staff recruited 13 sites (listed in Table 1.3), each of which they thought could (1) meet the JOBSTART program guidelines with little or no technical assistance except on techniques of client outreach and retention; (2) assemble sufficient operational funding for the full array of JOBSTART services (a significant barrier, as discussed above); and (3) yield a target of 200 sample members.[40] All had experience running programs that included some or all of the components of the JOBSTART model or working with young dropouts.

While all agreed to implement the JOBSTART model, the sites brought to the demonstration varying operating experiences, as shown in Table 1.3:

- **Sponsoring organizations.** The participating organizations included adult vocational schools, a community college, community-based organizations that focus on literacy development and GED preparation, community-based organizations that focus on occupational skills training, and the nonresidential components of three Job Corps Centers.[41]

- **Prior service emphasis.** Some sites previously had offered only basic education and no skills training, while others had offered both but had emphasized skills training. The education-focused sites may have attracted youths who were primarily interested in basic education rather than skills training. Similarly, some sites with strong histories of skills training may have attracted youths who were primarily interested in learning the skills needed for a particular occupation rather than attaining a GED.

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[40] See Auspos, 1987, and Auspos et al., 1989, for a detailed discussion of the characteristics of the sites in the demonstration.

[41] The Job Corps Centers operated their usual nonresidential programs. Thus, they offered all JOBSTART services plus other Job Corps services that are not part of the JOBSTART model.

TABLE 1.3

THE JOBSTART SITES

Agency Name	Location	Type of Organization	Prior Service Emphasis[a]	JOBSTART Program Structure[b]
Allentown Youth Services Consortium[c]	Buffalo, NY	Community-based	Education	Sequential/brokered
Atlanta Job Corps	Atlanta, GA	Job Corps Center	Education and training	Concurrent
Basic Skills Academy (BSA)	New York, NY	Community-based	Education	Sequential/brokered
Capitol Region Education Council (CREC)	Hartford, CT	Community-based	Education	Sequential/brokered
Center for Employment Training (CET)	San Jose, CA	Community-based	Training with some education	Concurrent
Chicago Commons Association's Industrial and Business Training Programs	Chicago, IL	Community-based	Training	Concurrent
Connelley Skill Learning Center	Pittsburgh, PA	Adult vocational school	Education and training	Concurrent
East Los Angeles Skills Center	Monterey Park, CA	Adult vocational school	Education and training	Concurrent
El Centro Community College Job Training Center[d]	Dallas, TX	Community college	Education and training	Sequential/in-house
Emily Griffith Opportunity School (EGOS)	Denver, CO	Adult vocational school	Education and training	Concurrent
Los Angeles Job Corps	Los Angeles, CA	Job Corps Center	Education and training	Sequential/in-house
Phoenix Job Corps	Phoenix, AZ	Job Corps Center	Education and training	Concurrent
SER/Jobs for Progress	Corpus Christi, TX	Community-based	Training	Concurrent

(continued)

TABLE 1.3 (continued)

NOTES: [a]"Education" refers to basic education, often as preparation for the GED examination. "Training" refers to instruction in occupational skills needed for specific jobs.

[b]*Concurrent* programs offer basic education and occupational training concurrently from the beginning of participation. *Sequential/in-house* programs offer basic education followed by occupational training, with both components provided in-house by the agency. *Sequential/brokered* programs provide basic education and then serve as a broker for occupational training, referring participants to other agencies.

[c]In October 1990 this site was renamed The Clarkson Center, Inc.

[d]In September 1988 this site was renamed the Edmund J. Kahn Job Training Center.

This diversity among sites led them to implement the basic JOBSTART program components in several ways. Eight sites were able to offer both education and training in-house and chose to provide them concurrently, with participants active in both activities from the start. Two sites provided both activities in-house, but offered them in sequence, with skills training following education. The remaining three sites did not have the capacity to offer skills training and chose to provide basic education themselves and work with other agencies to place their participants in subsequent occupational training elsewhere.

B. The Program Guidelines

Drawing on the lessons of the Job Corps and applying them within the constraints of JTPA, the demonstration developed a new alternative program offered in a nonresidential setting with fewer support services available to participants. The key elements, shown earlier in Table 1.1, included the core components of the Job Corps (basic education, occupational training, and job search) but a less extensive system of support services and no paid work experience.[42] In some respects (the definition of the target population and the requirement that certain activities be included), the program model was quite specific, while in others it allowed for considerable variation. The model set requirements as to the type and intensity of education and training services that were to be offered to participants, and it placed strong emphasis on the need for strategies to increase program retention. However, as mentioned earlier, sites were given a great deal of flexibility in implementing these core requirements.

1. Target group. Since the program was designed to reach a population largely unserved by existing programs, eligibility requirements were quite specific. Participation was limited to school dropouts who were between 17 and 21 years of age, did not have a high school diploma or GED, read below the eighth-grade level, and satisfied the JTPA definition of "economically disadvantaged" (defined primarily by household income or receipt of public assistance). Recognizing that program operators needed to meet enrollment and performance standard targets, however, the guidelines allowed for up to 20 percent of participants to read at or above the eighth-grade level.

2. Education and training. The demonstration sought to test an intervention that would be relatively intensive and lengthy compared to the usual JTPA activities and that would address the multiple deficits in participants' skills. As a result, the program model required sites to offer a specified minimum amount of both basic education and occupational training to provide the young people with a real opportunity to enhance their skills.[43] The 200-hour minimum of education was based on an estimate of what would be needed to bring the basic skills of most participants reading below the eighth-grade level up to the point where they could qualify for a GED or enter occupational skills training. The 500 hours of training was a compromise between the very lengthy training that research suggested was useful and what was practical in most JTPA environments.

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[42] Chapter 3 of this report provides more detail on the JOBSTART program model as implemented by the sites in the demonstration. A fuller description is given in Auspos et al., 1989.

Given the difficulty of keeping young people engaged in a program for an extended period and the competing demands on their time (including their need for income and their child care responsibilities), staff recognized that not all participants would complete these activities and that the total time in the program would be a year or less.

The occupational skills component required classroom rather than on-the-job training, in the belief that participants would benefit from the intensive, closely supervised instruction possible in a classroom setting. Again, no specific curriculum was required. Recognizing the advantages of applying learning to practical problems, however, the program model required that the training include a combination of theory and hands-on experience. Seeking to increase the chances of placement following training, the program model required that the training prepare participants for jobs in high-demand occupations and be developed in cooperation with local representatives of the private sector.

3. **Support services.** Attracting and keeping disadvantaged youths in education and training programs is a challenging problem, and the sites were expected to assist participants with transportation and child care. They were also encouraged to develop a package of other support services to facilitate program participation; the Job Corps sites offered considerably more support services than did the others.[44]

4. **Job placement assistance.** The guidelines required sites to identify possible training-related jobs for participants and to assist them in securing employment, but were not specific about how this should be done. All sites instructed the youths on work disciplines, employer expectations, and job search techniques, but the intensity of this effort ranged from informal guidance by counselors and other staff to more than 50 class hours in one site. Seven sites offered some form of work experience or internship (both paid and unpaid) to improve job skills. All sites provided

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[43] The program model did not specify any particular curricula, though it did encourage – but not require – sites to offer computer-assisted instruction.

[44] Job Corps Centers offered health services, recreational activities, and on-site food service, and more intensive counseling and peer support than did most other sites.

assistance in seeking employment when the youths left the program, although in two of the three sequential/brokered sites (CREC in Hartford and BSA in New York City), the responsibility fell solely on the training provider. This arrangement for job search assistance proved a serious limitation since, as will be discussed in Chapter 3, many young people did not reach the training phase in sequential/brokered sites.

C. Key Dimensions of Program Variation

Although the demonstration did test the JOBSTART program model, this discussion has made clear that there was considerable variation across the sites. JOBSTART was not the same program for everyone. The analysis presented in this report seeks to understand how JOBSTART "worked" differently for subgroups within the sample and at different types of sites.

1. Subgroups of the sample. Much of the analysis presented in this report describes differences in program implementation and impacts for subgroups defined based on characteristics of individuals when they entered the program: for example, gender; ethnicity; age; prior education, work experience, and criminal record; parenting status; and welfare receipt. In this evaluation, central subgroup splits are males versus females (because of differences in prior employment and earnings), and – among females – women who were living with children of their own when they entered the program versus other women (because of different child care responsibilities).

2. Types of sites. The previous report on the implementation of JOBSTART (Auspos et al., 1989) highlighted two dimensions of local variation as important influences on the program experience of the JOBSTART youths:[45]

- **Concurrent versus sequential education and training.** Programs could offer youths basic education classes and vocational skills instruction at the same time (a concurrent model) or basic education before skills training (a sequential model).

- **In-house versus brokered services.** Programs could offer youths education and training at the same agency, or the agency providing basic education could serve as a broker, helping participants who were completing the education phase to find appropriate training at other institutions (sequential/brokered sites).

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[45] The report also highlighted variations among the sites: whether they were serving JOBSTART youths in mainstream adult classes or in separate classes for youths; whether they offered computer-assisted instruction; and how they differed in scheduling (that is, the number of hours a day devoted to various activities) and in the length of their courses. Since these did not appear to have a major influence on program implementation or on participation by the youths, they were unlikely to have affected impacts and are not emphasized in this report.

Recent research has drawn policymakers' attention to the implications of sequential versus concurrent programs and to the extent to which education and training in concurrent programs are truly integrated. The Minority Female Single Parent (MFSP) Demonstration, funded by The Rockefeller Foundation, tested different models of education and training in four local agencies.[46] The evaluators argued that the one program among the four in the study with the most consistent positive impacts on employment and earnings (the Center for Employment Training [CET] in San Jose) probably achieved this result because of programmatic and organizational features that distinguished it from the remaining three.[47] Specifically, the researchers highlighted that "the training design of the CET program – which emphasized training for all regardless of educational skill levels – offered remedial education within the context of job skill training, and accommodated trainees with diverse levels of educational skills."[48]

While the difference between sequential and concurrent programs is obvious, in practice, there is no clear distinction between integrated concurrent programs and other concurrent programs; they form a spectrum rather than falling into two neat categories. Among the JOBSTART sites, for example, CET/San Jose operated the most integrated program, with basic skills instruction being offered within the context of vocational training. Chicago Commons, another site, operated a partially integrated program and offered several training courses requiring technical knowledge and mathematics skills. Even after imposing entrance requirements among the most stringent in any JOBSTART site, Chicago Commons found that the skills deficiencies of some participants were too severe to be addressed within the integrated training context. Thus, the site also offered a separate basic education class. Other concurrent sites such as SER/Corpus Christi and Connelley in Pittsburgh operated separate education classes aimed at preparing people to pass the GED examination.[49]

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[46] See Gordon and Burghardt, 1990, and Burghardt et al., 1992.

[47] The CET site in the MFSP demonstration enrolled minority female single parents, whose average age was 28, and served them in San Jose and several other East Bay communities. The San Jose program was also a site in JOBSTART, but only 10 mothers became part of the JOBSTART sample at CET/San Jose, and they were considerably younger than the MFSP sample.

[48] Gordon and Burghardt, 1990, p. xxvi. The authors also cited a number of other factors that were unique to CET among the four sites and that they believed contributed to its large impacts, including its financial stability and experienced staff; integration of the MFSP program into an ongoing training operation with a similar mission; large scale, which allowed for training in a variety of occupations in demand in the local economy; attention paid to job placement; and availability of on-site child care. The remaining three sites in the MFSP demonstration emphasized "the acquisition of basic skills before entry into job skill training" (p. xxvi) – that is, "sequential programs" using the terminology in this report.

[49] These sites tried to coordinate the activities in education and training classes via conferences among the instructors and inclusion of basic skills instruction in some training classes.

As discussed earlier, the JOBSTART sites included agencies that provided all services in-house and others where the sponsoring organization served as a broker, arranging some services for participants at other organizations. When these two site groupings are combined, this yields three types of sites (sequential/in-house, sequential/brokered, and concurrent), which are analyzed separately in Chapter 3, on JOBSTART services.

There is a serious complicating factor in analyzing the reasons for differences in program implementation and impacts across sites or groups of sites: The young people were not randomly assigned to different programmatic approaches. Instead, the sites offering these sequential/in-house, sequential/brokered, and concurrent programs operated in different kinds of settings and local labor markets[50] and served different types of youths with varying backgrounds and interests.[51] Thus, the structure of the program was not the only difference among sites. Given these many differences, it is impossible to isolate the influence of one factor – such as concurrent versus sequential program structure – on program effectiveness.[52] As discussed in more detail in Chapter 2, caution must therefore be exercised in making cross-site comparisons of program implementation and impacts.

IV. The JOBSTART Evaluation and the Organization of This Report

The evaluation of JOBSTART is divided into three main components. The first deals with the sites' implementation of the program. *Launching JOBSTART*, the initial report on the demonstration, discussed site selection and characteristics, the operation of the program within JTPA, and early implementation experiences.[53] A second report, *Implementing JOBSTART*, completed the implementation analysis by describing the content of JOBSTART activities, the participation patterns of the young people in the program, and operational lessons to be drawn from the demonstration.[54]

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[50] For example, the JOBSTART programs operated in very different labor markets. The unemployment rates in the sites' metropolitan areas varied from a low of 3 percent in 1987 in Hartford, where CREC was located, to 12 percent in 1986 in Corpus Christi, where SER operated. Youth unemployment rates varied from 6 percent in 1986 in Hartford to 27 percent in 1985 in New York City, where BSA was located.

[51] Chapter 3 presents information on the characteristics of the sample in each site.

[52] To rigorously compare the impacts of different programmatic approaches, more than one approach would have to be operated in each site, and youths would have to be randomly assigned to one of them. Even with this design, if the programs differed on several dimensions, it would still be impossible to isolate the effect of any one dimension. This type of research has rarely been undertaken. Examples include MDRC's study of the impacts of job search alone versus job search plus community work experience in San Diego (Goldman et al., 1986), Mathematica Policy Research's study of alternative reforms of the Unemployment Insurance system (Corson et al., 1989), and MDRC's ongoing study of the Job Opportunities and Basic Skills Training (JOBS) Program, which involves comparisons of various service strategies.

[53] Auspos, 1987.

The second component of the evaluation is an analysis of program impacts. Findings based on two years of follow-up were reported in *Assessing JOBSTART*, while final impact estimates based on four years of follow-up form the core of this report. The research was designed to separate out the effects of JOBSTART itself from events attributable to other factors (such as other services participants were receiving and events in their lives outside the program). To accomplish this, all people who applied for JOBSTART and were found to be eligible were randomly assigned to either an experimental or a control group. Those in the experimental group were given access to the JOBSTART program services; those in the control group were not, although they could receive other services offered in their community. Since the youths were assigned at random to the two groups, they were similar except for the fact that only the experimental group could receive JOBSTART services. This type of analysis is often called "experimental" research because of its reliance on the methods of classical scientific experiments. Individuals in both groups were scheduled to be surveyed 12, 24, and approximately 48 months after being randomly assigned. (The time frame for applying to JOBSTART varied from site to site but ranged overall from August 1985 through November 1987. Hence, the fielding of each wave of the survey also extended over many months.) Using these surveys, the experiences of the two groups can be compared to estimate the effect of the program on educational attainment, employment, earnings, use of public benefits, and other outcomes.

Estimates of the impacts of the program for subgroups of the sample defined based on pre-random assignment characteristics are also straightforward, since the control group counterparts of members of the experimental group can be identified. For example, in a subgroup analysis by gender, males in the experimental group are compared to males in the control group.

The third component of the evaluation assesses the cost-effectiveness of the program. *Assessing JOBSTART* included an analysis of the costs of providing the JOBSTART services and the sources of variation across the sites. Chapter 7 of this report summarizes this analysis and presents an analysis of the net benefits and costs of JOBSTART from a variety of perspectives.

As a guide to what is ahead, Chapter 2 of this final report on JOBSTART presents the key research questions and the research design used to address them. It also includes information showing that the sites did succeed in recruiting the young, economically and educationally disadvantaged youths targeted by the demonstration. Chapter 3 discusses the implementation of the

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[54] Auspos et al., 1989. These findings are summarized and updated in *Assessing JOBSTART* (Cave and Doolittle, 1991).

JOBSTART program, concluding that most youths in JOBSTART participated in education and training activities more than the typical participant in JTPA-funded programs, and nearly as much as the typical Job Corpsmember. Here, as in all subsequent chapters, the report seeks to understand the overall results by examining whether and how JOBSTART operated differently for key subgroups of youths in the sample defined by pre-random assignment characteristics. Chapter 4 examines educational outcomes, especially the degree of experimentals' participation in education and training compared to that of controls, and whether JOBSTART led to increased attainment of a high school diploma or GED during the four years of follow-up. Chapter 5 examines the indications of how this investment in "human capital" affected youths' employment and earnings, while Chapter 6 presents impacts on other outcomes such as welfare receipt, arrests, childbearing, and "productive activity," defined as time spent working or in education or training. An analysis of the net costs of JOBSTART and the benefits produced by the program is summarized in Chapter 7. In conclusion, Chapter 8 discusses the implications of the JOBSTART evaluation for future program design, operation, and research.

CHAPTER 2

THE JOBSTART EVALUATION AND SAMPLE

This chapter describes the research design and the sample of young school dropouts who were involved in the study. Section I indicates how the study was designed to answer the research questions posed in Chapter 1, with a special emphasis on the random assignment approach for assessing the difference the program made in the lives of the young people who participated in JOBSTART. Section II describes the characteristics of the research sample overall and of different subgroups of the sample, defined by their site, gender, and other characteristics of interest to policymakers and program planners.

I. An Overview of the Study Design

Although education and training services for young school dropouts are limited, some youths who entered JOBSTART would have received a high school diploma or GED certificate, found a job, increased their earnings, or left the welfare rolls on their own even if they had not been in the program. As noted in Chapter 1, to isolate the impact of JOBSTART from other factors that might have produced such outcomes, MDRC randomly assigned applicants to experimental and control groups. The two groups were similar except that only the experimental group could receive JOBSTART services. Comparison of the two groups' experiences during the four years after random assignment (the follow-up information available for this report) thus provides a reliable estimate of the difference that the program made.[1]

A. How Random Assignment Was Conducted

Figure 2.1 shows the steps in the client intake and random assignment process.[2] Youths who expressed an interest in program services entered the program through a process that took from one

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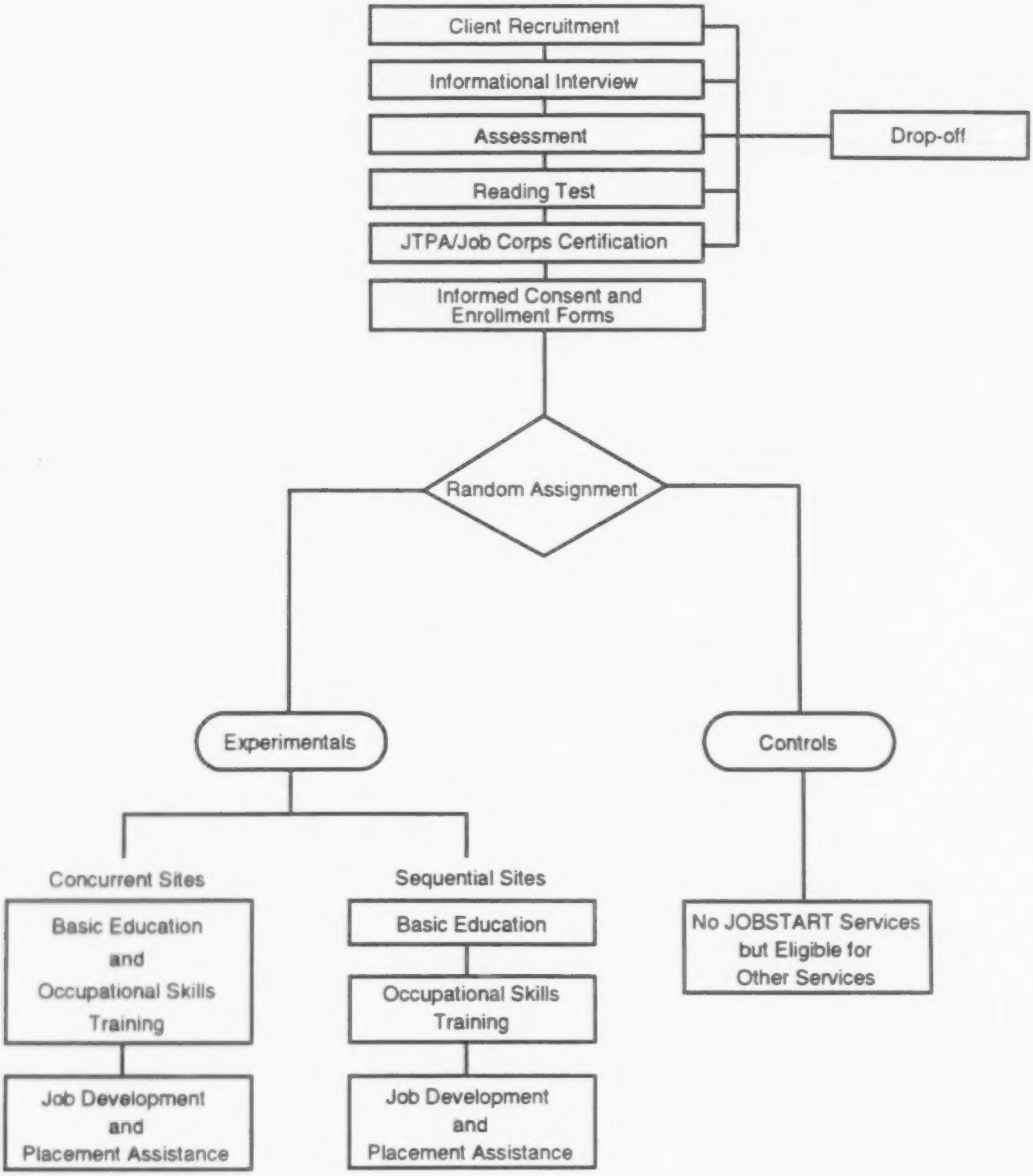
[1] Appendix A details the sources of data used in comparing the experiences of the two groups: enrollment forms completed just prior to random assignment; a management information system (MIS) that provided data on participation in the program; results from the Test of Adult Basic Education (TABE) administered to members of the experimental group; follow-up surveys for this evaluation conducted 12, 24, and 48 months after random assignment; data on program costs from a variety of sources; and qualitative data based on interviews with program staff, field observations of program operations, and focus group discussions with participants.

[2] For more detail see Auspos et al., 1989.

FIGURE 2.1
THE JOBSTART EVALUATION DESIGN AND SAMPLE FLOW


Client Recruitment
Informational Interview
Assessment
Reading Test
JTPA/Job Corps Certification
Drop-off
Informed Consent and Enrollment Forms
Random Assignment
Experimentals
Controls
Concurrent Sites
Sequential Sites
Basic Education and Occupational Skills Training
Job Development and Placement Assistance
Basic Education
Occupational Skills Training
Job Development and Placement Assistance
No JOBSTART Services but Eligible for Other Services

SOURCE: Adapted from Auspos et al., 1989.

NOTE: Support services such as child care and transportation were offered at both concurrent and sequential sites.

day to one month (10 days on average), depending on the site.[3] Most of the steps were part of the usual JTPA Title IIA (or, in Atlanta, Los Angeles, and Phoenix, the Job Corps) intake procedures; in most sites only the reading test and random assignment were added for the JOBSTART Demonstration. The order of the steps varied from site to site, as did the division of responsibility between the JOBSTART program operator and the local JTPA service delivery area (SDA). The process included:

- **Client recruitment.** JOBSTART was voluntary, so the JOBSTART program operator and, in some cases, the local SDA actively recruited youths to apply, using a variety of techniques to meet their enrollment goals. Program staff approached potential recruits through media announcements; mailings to dropouts and welfare recipients; and outreach visits to schools, parks, and other youth gathering places. They distributed posters and fliers advertising program services and sought referrals of eligible youths from JTPA, community organizations, schools, and social service agencies. Recruitment activities frequently took staff members beyond the walls of their office and their nine-to-five workday. Recruitment through public school referrals or outreach was productive in the school-based JOBSTART programs.

- **Informational interview.** In a brief interview, JOBSTART staff would explain to a potential applicant the program's services and obligations and, often, the random assignment procedures. Some sites also regularly included a tour of their facilities to help recruits understand program services, opportunities, and demands.

- **Assessment.** Program staff assessed whether applicants met the age (17 to 21), educational status (school dropout), and income requirements for JOBSTART. They also ascertained the youths' needs for support services and their appropriateness for the program, screening out those with problems the program was not equipped to handle. The assessment process was relatively extensive at the sites operated by Job Corps, which had the broadest array of support services. Job Corps staff assessed recruits for emotional problems, drug and alcohol abuse, trouble with the law, unstable living situations, health problems, and motivation. Other sites screened mostly to identify youths who were likely to prove dangerous or disruptive, such as those with evident drug or alcohol problems.

- **Reading test.** Most program operators tested recruits early in the intake process to determine whether their reading skills were below the eighth-grade level, as required by JOBSTART eligibility criteria. Four sites (the three Job Corps Centers and CET/San Jose) delayed testing until later in the program, limiting their testing to participants. As noted earlier, all the sites were permitted to enroll up to 20 percent of their recruits with higher reading scores to help meet enrollment goals. Some sites also set a lower limit of a fourth-, fifth-, or sixth-grade reading level. Staff at those sites believed that the youths would need to read at least at those levels to benefit from the education and training services that were available locally.

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[3] Sites varied greatly in the amount of initial assessment they conducted before allowing entry into the program. There was also wide variation in state and local interpretation of the documentation needed to establish eligibility for JTPA Title IIA programs.

- **JTPA Title IIA/Job Corps certification.** Recruits had to prove that they met the eligibility criteria for JTPA Title IIA-funded services. At the three Job Corps sites recruits also had to meet Job Corps eligibility criteria. At all the sites certification of eligibility required proof of residency, age, and economic disadvantagedness. SDAs at most sites required applicants to provide supporting documentation of all aspects of JTPA Title IIA eligibility for approval of enrollment into JOBSTART. Local regulations and practices concerning the certification process strongly influenced the speed and ease of certification. In fact, program operators at six sites pinpointed JTPA Title IIA certification procedures as a major bottleneck in the intake and enrollment process.

- **Informed consent form, enrollment form, and random assignment.** After a staff member described the random assignment process, the applicant would sign an informed consent form, agreeing to accept the results of random assignment and to cooperate in follow-up survey interviews. At that point, program or SDA staff filled out the enrollment form, using information provided by the applicant, and then telephoned MDRC, where the random assignment was made. Youths assigned to the experimental group were told to report to classes or, in some sites, to an orientation session. Program staff would contact experimental group members ("experimentals") who did not appear for program activities, encouraging them to participate and assisting them with needed support services. Applicants assigned to the control group would be reminded that they were part of the research project and would be contacted later for the follow-up surveys. They were also told that they could seek services elsewhere on their own.[4]

A total of 2,312 people were randomly assigned: 1,163 to the experimental group and 1,149 to the control group.[5] The sites conducted random assignment over varying time periods. Connelley Skill Learning Center in Pittsburgh enrolled the first sample members in August 1985, and the Los Angeles Job Corps enrolled the last sample members in November 1987. Open-entry/open-exit sites continuously recruited applicants to maintain enrollment levels, while sites operating fixed-cycle programs intensified recruitment efforts before the start of a cycle of classes. Overall, sites reported that about 89 percent of the 1,163 youths in the experimental group participated to some extent in JOBSTART services. The percentage participating did vary among the sites, from a high of 100 percent at Allentown in Buffalo and El Centro in Dallas to a low of 64 percent at CET/San Jose. (Chapter 3 presents more detailed information on participation by site.) Four factors influenced the percentage of experimentals reported to be active in the program:

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[4] In many sites, program staff would often provide sample members with a list of other services in the community at the time of random assignment. Individuals who ended up in the control group may have used that list to seek alternative services. As discussed later in this chapter and elsewhere in the report, however, the experimental group had much higher levels of participation in services than did the control group during the first two years of follow-up.

[5] All but one of the 2,312 youths who were randomly assigned had completed enrollment forms, which provided the pre-program baseline data on age, gender, previous employment, extent of schooling, and other characteristics that were used in the evaluation. Such data were used to define key subgroups within the full sample and to adjust the impact estimates for any experimental-control group differences in baseline characteristics. In the remainder of this report, we therefore cite 2,311 as our "full research sample."

- **Length of the intake process.** The process of recruitment into the JOBSTART Demonstration took a relatively short time in many sites, often less than a week. At a few sites, however, the extended checks of eligibility (particularly at the Job Corps sites) meant that intake lasted much longer, and in the meantime some youths who were eventually assigned to the experimental group found other program opportunities or lost interest.

- **Open-entry/open-exit versus fixed-cycle scheduling.** Open-entry programs allow young people to enter and finish at any time, while other programs operate on fixed schedules of class cycles.[6] Youths assigned to the experimental group in fixed-cycle sites might face delays in program start-up, resulting in lower participation rates.

- **Start-up or scheduling problems.** Some sites had unexpected problems getting youths into services. The most notable example was the experience of the early entrants at CET/San Jose, where program slots were not available for up to a month after random assignment because of funding cuts. This delay contributed to this site's experimentals having the lowest rate of participation in JOBSTART services, although, as shown in Chapter 4, the experimental-control difference in service receipt was as great at CET as it was for the entire sample.

- **Differences in the sites' attendance reporting.** The program elements counted in participation at all the sites were education, training, and other activities such as life skills training, work experience, and – in the Job Corps sites – a lengthy orientation. Participation in an extended assessment of training interests was not included in hours at CET/San Jose, however. As a result, if youths at CET attended this assessment and nothing else, their reported number of hours was zero and they were counted as nonparticipants. This difference in reporting practice may have affected CET/San Jose's participation rate and reported hours in activities.

B. The Research Sample Used in This Report

Follow-up surveys conducted at 12, 24, and 48 months after random assignment gathered data on outcomes such as participation in education and training programs, educational attainment, employment, earnings, and receipt of public assistance.[7] Of the 2,311 youths in the full research sample, 1,941, or 84 percent, responded to the 48-month follow-up survey and constitute the "48-month impact sample" analyzed in this report.[8] The experience of the 988 experimentals in this sample also serves as the basis for the implementation analysis in Chapter 3, which examines such issues as participation rates in JOBSTART and its components, and hours and duration of participation.[9]

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[6] The sites operating open-entry/open-exit programs were Allentown in Buffalo, the Atlanta Job Corps, CET/San Jose, the East Los Angeles Skills Center, El Centro in Dallas, the Los Angeles Job Corps, and the Phoenix Job Corps. EGOS in Denver offered classes on a semester schedule but allowed entry whenever classes were in session. The remaining five sites – BSA in New York, CREC in Hartford, Chicago Commons, Connelley in Pittsburgh, and SER/Corpus Christi – operated fixed-cycle programs.

C. Key Methodological Issues for the Impact Analysis

For this study to produce unbiased estimates of program impacts, several conditions had to be met. These are addressed in the following questions.[10]

1. Did random assignment result in a group of experimentals with the same measured pre-program characteristics as the control group? Random assignment – if properly implemented – should create a group of JOBSTART experimentals with the same characteristics at the start of the program as the controls, on average, so that any observed differences between the experimentals and the controls in post-random assignment behavior will provide unbiased estimates of program impacts.[11] The information presented in Appendix B (Tables B.1 and B.2) for the 1,941 people randomly assigned for whom there were 48 months of follow-up data shows that there were virtually no measured differences in characteristics between the experimentals and controls.[12]

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[7] As noted in Appendix A, information on participation in JOBSTART was provided by the sites as part of a special management information system (MIS) created for the demonstration. The follow-up surveys collected information on participation in all other education, training, and employment programs for both experimentals and controls.

[8] This impact sample is larger than that responding to the 24-month follow-up survey (1,839, or 80 percent of the full sample). Some individuals not responding at 24 months did respond at 48 months, at which time they responded to questions on the earlier period.

[9] The 48-month impact sample for this report is slightly different from the sample in our previous reports on JOBSTART because some who responded to the earlier surveys did not respond to the final 48-month survey, and some who responded to the final survey did not respond to an earlier survey. Thus, the findings in this report on program participation and impacts during the first 24 months of follow-up are slightly different from those presented in our earlier reports. Nevertheless, the shifts in sample have not materially affected the basic story.

[10] For a fuller discussion of some of these issues, see Appendix B.

[11] This condition is known as the "internal validity" of the estimates.

[12] The only difference that was statistically significant was that experimentals in the 48-month impact sample were slightly more likely than controls to be a part of an AFDC case headed by another member of their household (see Table B.2).

On a site-by-site basis, the 48-month impact sample consists of 26 separate groups of experimentals and controls. If experimentals in any site are compared with controls in that site, the internal validity of site impacts may be assessed. As would be expected in 13 relatively small subsamples of the full 48-month impact

(continued...)

2. Are those 1,941 sample members with 48 months of survey data representative of the entire JOBSTART sample of 2,311? Forty-eight months of survey data are available for nearly 84 percent of all the youths who were randomly assigned, including 85 percent of experimentals and 83 percent of controls, 88 percent of women who were custodial mothers at baseline, 88 percent of other women, and 80 percent of males. Appendix Table B.3 shows that there are some statistically significant differences between those who responded to the surveys and nonresponders, but in characteristics other than experimental status. In the full sample, responders were less likely than nonresponders to be male. Responders were more likely than nonresponders to have entered the sample at the Allentown (Buffalo), CREC (Hartford), or El Centro (Dallas) sites, to be white or Hispanic, to be older, to have left school during grade 11 or 12, and to have lived with both parents at age 14.

When nonresponse is randomly distributed among members of both the experimental and control groups, it is troublesome only because it reduces the sample size and thus the statistical power to find impacts of a given magnitude.[13] However, when nonresponse is greater among one research group (which is not the case here) or among members of either research group who have certain characteristics (such as men), impacts may be biased slightly unless they are corrected for nonresponse. The impacts presented in this report do not include any corrections for the differences between survey responders and nonresponders.[14] The success of attempts to implement such corrections is uncertain, and the differential response rates found do not seem large enough to warrant such measures, which could introduce biases of their own. The high overall response rate of 84 percent makes findings on the 48-month impact sample representative of a very broad group of the full sample.

3. Did most experimentals receive JOBSTART services, and did relatively few controls receive them or any equivalent services? For these conditions to be met, experimentals must participate in JOBSTART and controls must be excluded from JOBSTART and not find equivalent services elsewhere in their community. As discussed above, nearly 90 percent of experimentals were active in JOBSTART. Because of successful implementation of random assignment procedures,

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[12](...continued)
sample of 1,941, there are a few experimental-control differences in demographic characteristics within individual sites.

[13] Randomly distributed nonresponse does not alter the expected values of adjusted mean outcomes, and thus does not bias impacts.

[14] The most flexible correction for nonresponse is incorporation of an additional equation for survey response into a two-equation system with the impact equation.

virtually no controls were served in JOBSTART programs. In addition, analysis of the receipt of education and training services from JOBSTART and non-JOBSTART sources indicates that controls did not find an equivalent level of services elsewhere (see Chapter 4). For example, in the first year after random assignment, 90 percent of experimentals but only 26 percent of controls participated in some type of education or training activity. As this and the other measures used in Chapter 4 indicate, experimentals did receive a noticeably greater total amount of employment and training services. But it is important to keep in mind that controls were not an unserved group; many received substantial services from sources other than the JOBSTART programs. Therefore, the impact findings presented in Chapters 4 through 6 of this report should be interpreted as measuring the incremental impact of the services received by experimentals above those received by controls.

4. Do the impacts per person assigned to the experimental group differ greatly from the impacts per person participating in JOBSTART? Some of those who were randomly assigned to the experimental group (the group given access to the JOBSTART program) never participated. However, they still were included as part of the experimental group when average impacts were calculated, somewhat "diluting" the impacts.[15] Fortunately, the percentage of nonparticipants was small (only 11 percent of the 988 experimentals in the 48-month impact sample), so including them diluted the impacts only slightly.[16] In other words, while the impacts refer to all surveyed experimentals (nonparticipants as well as participants), they would be only slightly changed if adjusted to include surveyed participants only.[17]

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[15] If the nonparticipants had not been counted, the experimental group would no longer have been truly comparable to the control group. Including them in the impact calculations was designed to avoid a form of "selection bias" – in this case, caused by those who had "selected themselves" out of their chance to join the JOBSTART program or were discouraged by program staff.

[16] See Appendix B for details on such adjustments. In some sites, nonparticipation rates were considerably higher than the 11 percent for the entire 48-month impact sample, so the difference between impacts per experimental and per participant is greater.

[17] In addition to the issues discussed above, the study posed some important methodological issues concerning data collection. Most important among them was the problem of abrupt changes in some key variables at the "seam" between survey waves. For example, employment and earnings reported for month 12 in the 12-month survey were noticeably higher than employment and earnings for month 13 in the 24-month survey. A similar shift occurred at the month-25 seam between the 24- and 48-month surveys. This problem is likely to have arisen owing to recall problems in the later surveys (at 24 and 48 months), since the respondents were providing information on a period one or two years in the past. Sample members who were "missed" in the 24-month survey and provided information on the entire 48-month period at one time did not exhibit an abrupt change in employment and earnings levels. Appendix A discusses how this problem was handled in the JOBSTART evaluation. Similar problems have been encountered in other studies; see Burghardt et al., 1992, Appendix D, for another example.

II. Characteristics of the JOBSTART Youths

Examining the pre-program experiences and characteristics of the young people in the JOBSTART sample is important for three reasons. First, it shows whether the sites succeeded in enrolling economically disadvantaged young people with poor skills who were the target group for the demonstration. Second, it permits a comparison of the JOBSTART youths with those served by other federally funded employment and training programs. Third, much of the analysis in this report moves beyond results for the full sample of JOBSTART youths to examine whether and how the program worked differently for subgroups of the sample (especially males, custodial mothers, and all other women), and understanding the pre-program characteristics of these subgroups is the first step in such an analysis. This third reason is important because subgroups defined by a single characteristic (such as gender, age, prior employment, or the type of program model at the site to which they applied) may vary in other characteristics as well. Females in the sample, for example, may have had less prior employment experience and more prior public assistance receipt than the men in the sample. Sites offering education followed by training at another agency may attract different applicants than those known for their training courses. Understanding the combination of characteristics associated with subgroups helps prevent misinterpretation of any observed differences in program participation and effectiveness. With this goal of the report in mind, the remainder of this chapter summarizes the characteristics of the youths at each site and of key subgroups that will be examined later in the report.

A. Characteristics of the 48-Month Impact Sample

Table 2.1 provides detailed background information on the 48-month impact sample of JOBSTART youths and indicates that the sites in the demonstration succeeded in recruiting the intended target group. The column labeled "all 13 sites" shows the characteristics of the entire impact sample; the remaining columns are discussed in Section IIB of this chapter. The sample is made up of slightly more women than men; most of the sample are members of minority groups and were unmarried at baseline; nearly 75 percent were under 20 years of age at baseline; slightly less than half had not worked during the year prior to random assignment; and about 60 percent left school before the 11th grade.[18]

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[18] The only real divergence of the sample from the intended target group occurred because a slightly higher than planned percentage of the youths read at the eighth-grade level or above. This happened because some sites tested reading skills after random assignment and only for experimentals. For this reason, sample members' reading levels are not included among the baseline characteristics in this report.

TABLE 2.1

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY SITE

Characteristic and Subgroup	All 13 Sites (%)	Concurrent Atlanta Job Corps (%)	CET/ San Jose (%)	Chicago Commons (%)	Connelley (Pittsburgh) (%)	East LA Skills Center (%)	EGOS (Denver) (%)	Phoenix Job Corps (%)	SER/ Corpus Christi (%)
Gender									
Women	53.6***	59.4	49.7	41.3	54.3	42.5	66.7	55.2	42.5
Men	46.4	40.6	50.3	58.7	45.7	57.5	33.3	44.8	57.5
Ethnicity									
White, non-Hispanic	8.9***	2.9	15.0	6.7	8.7	0.9	11.1	23.9	8.9
Black, non-Hispanic	44.3	97.1	6.0	74.7	91.3	0.0	28.3	17.2	4.9
Hispanic	43.6	0.0	70.1	18.7	0.0	95.3	58.1	53.7	86.2
Other	3.2	0.0	9.0	0.0	0.0	3.8	2.5	5.2	0.0
Ethnicity, by gender									
Women									
White, non-Hispanic	5.0***	2.9	9.0	4.0	3.3	0.9	5.6	13.4	4.9
Black, non-Hispanic	24.1	56.5	2.4	26.7	51.1	0.0	23.2	5.2	3.2
Hispanic	23.2	0.0	37.1	10.7	0.0	39.6	35.9	34.3	34.4
Other	1.3	0.0	1.2	0.0	0.0	1.9	2.0	2.2	0.0
Men									
White, non-Hispanic	3.9	0.0	6.0	2.7	5.4	0.0	5.6	10.4	4.0
Black, non-Hispanic	20.2	40.6	3.6	48.0	40.2	0.0	5.1	11.9	1.6
Hispanic	20.4	0.0	32.9	8.0	0.0	55.7	22.2	19.4	51.8
Other	1.9	0.0	7.8	0.0	0.0	1.9	0.5	3.0	0.0
Parental status									
Women living with own child(ren)									
No	27.5***	27.5	44.3	20.0	22.3	30.2	29.8	24.6	18.2
Yes	26.2	31.9	5.4	21.3	32.1	12.3	36.9	30.6	24.3
Men who have own child(ren)									
No	40.4	36.2	45.5	40.0	35.9	55.7	30.3	39.6	48.2
Yes	5.9	4.3	4.8	18.7	9.8	1.9	3.0	5.2	9.3
Sample size	1,941	69	167	75	184	106	198	134	247

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup	All 13 Sites (%)	Sequential/In-House El Centro (Dallas) (%)	Sequential/In-House LA Job Corps (%)	Sequential/Brokered Allentown (Buffalo) (%)	Sequential/Brokered BSA (NYC) (%)	Sequential/Brokered CREC (Hartford) (%)
Gender						
Women	53.6***	53.1	58.9	58.5	47.9	64.6
Men	46.4	46.9	41.1	41.5	52.1	35.4
Ethnicity						
White, non-Hispanic	8.9***	6.7	3.5	14.1	3.4	4.0
Black, non-Hispanic	44.3	69.8	48.1	77.8	64.1	52.5
Hispanic	43.6	22.3	36.8	7.4	31.6	43.4
Other	3.2	1.1	11.7	0.7	0.9	0.0
Ethnicity, by gender						
Women						
White, non-Hispanic	5.0***	4.5	1.7	9.6	1.7	2.0
Black, non-Hispanic	24.1	34.1	27.7	43.7	27.4	33.3
Hispanic	23.2	14.5	23.4	4.4	18.8	29.3
Other	1.3	0.0	6.1	0.7	0.0	0.0
Men						
White, non-Hispanic	3.9	2.2	1.7	4.4	1.7	2.0
Black, non-Hispanic	20.2	35.8	20.3	34.1	36.8	19.2
Hispanic	20.4	7.8	13.4	3.0	12.8	14.1
Other	1.9	1.1	5.6	0.0	0.9	0.0
Parental status						
Women living with own child(ren)						
No	27.5***	21.8	30.7	25.9	29.1	36.4
Yes	26.2	31.3	28.1	32.6	18.8	28.3
Men who have own child(ren)						
No	40.4	42.5	39.8	29.6	51.3	29.3
Yes	5.9	4.5	1.3	11.9	0.9	6.1
Sample size	1,941	179	231	135	117	99

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup	All 13 Sites (%)	Concurrent Atlanta Job Corps (%)	CET/ San Jose (%)	Chicago Commons (%)	Connelley (Pittsburgh) (%)	East LA Skills Center (%)	EGOS (Denver) (%)	Phoenix Job Corps (%)	SER/ Corpus Christi (%)
Employed within past year									
No	47.1***	37.7	39.5	56.0	28.8	50.9	39.9	54.5	34.4
Yes	52,9	62.3	60.5	44.0	71.2	49.1	60.1	45.5	65.6
Prior employment, by gender									
Women employed within past year									
No	30.0***	30.4	23.4	26.7	18.5	25.5	31.8	38.1	24.3
Yes	23.6	29.0	26.3	14.7	35.9	17.0	34.8	17.2	18.2
Men employed within past year									
No	17.1	7.2	16.2	29.3	10.3	25.5	8.1	16.4	10.1
Yes	29.3	33.3	34.1	29.3	35.3	32.1	25.3	28.4	47.4
Left school in grade 11 or 12									
No	58.7***	59.4	39.5	48.0	65.2	67.9	56.1	61.9	74.1
Yes	41.3	40.6	60.5	52.0	34.8	32.1	43.9	38.1	25.9
Received occupational training within past year									
No	83.2***	69.6	91.6	88.0	66.3	84.9	93.4	94.0	69.2
Yes	16.8	30.4	8.4	12.0	33.7	15.1	6.6	6.0	30.8
Age									
16-19	73.4***	76.8	77.8	42.7	53.8	78.3	76.3	85.8	70.4
20 or 21	26.6	23.2	22.2	57.3	46.2	21.7	23.7	14.2	29.6
Marital status									
Ever married	9.5***	5.8	10.8	2.7	4.3	3.8	8.6	11.2	28.7
Never married	90.5	94.2	89.2	97.3	95.7	96.2	91.4	88.8	71.3
Living in own household or with boy/girlfriend									
No	81.1***	88.4	86.8	78.7	74.5	87.7	74.2	85.8	75.7
Yes	18.9	11.6	13.2	21.3	25.5	12.3	25.8	14.2	24.3
Sample size	1,941	69	167	75	184	106	198	134	247

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup	All 13 Sites (%)	Sequential/In-House		Sequential/Brokered		
		El Centro (Dallas) (%)	LA Job Corps (%)	Allentown (Buffalo) (%)	BSA (NYC) (%)	CREC (Hartford) (%)
Employed within past year						
No	47.1***	45.8	74.9	57.0	63.2	30.3
Yes	52.9	54.2	25.1	43.0	36.8	69.7
Prior employment, by gender						
Women employed within past year						
No	30.0***	25.7	46.3	40.0	31.6	24.2
Yes	23.6	27.4	12.6	18.5	16.2	40.4
Men employed within past year						
No	17.1	20.1	28.6	17.0	31.6	6.1
Yes	29.3	26.8	12.6	24.4	20.5	29.3
Left school in grade 11 or 12						
No	58.7***	68.2	41.1	54.8	58.1	69.7
Yes	41.3	31.8	58.9	45.2	41.9	30.3
Received occupational training within past year						
No	83.2***	89.4	90.9	81.5	77.8	83.8
Yes	16.8	10.6	9.1	18.5	22.2	16.2
Age						
16-19	73.4***	79.3	78.8	74.8	72.6	78.8
20 or 21	26.6	20.7	21.2	25.2	27.4	21.2
Marital status						
Ever married	9.5***	11.7	4.3	4.4	1.7	6.1
Never married	90.5	88.3	95.7	95.6	98.3	93.9
Living in own household or with boy/girlfriend						
No	81.1***	89.9	87.4	60.0	94.0	77.8
Yes	18.9	10.1	12.6	40.0	6.0	22.2
Sample size	1,941	179	231	135	117	99

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup	All 13 Sites (%)	Concurrent							
		Atlanta Job Corps (%)	CET/ San Jose (%)	Chicago Commons (%)	Connelley (Pittsburgh) (%)	East LA Skills Center (%)	EGOS (Denver) (%)	Phoenix Job Corps (%)	SER/ Corpus Christi (%)
Own AFDC case or receiving General Assistance									
No	73.1***	66.7	91.6	45.3	56.0	74.5	73.2	81.3	86.2
Yes	26.9	33.3	8.4	54.7	44.0	25.5	26.8	18.7	13.8
Own AFDC case									
No	78.4***	78.3	95.2	74.7	67.4	76.4	74.7	82.1	87.9
Yes	21.6	21.7	4.8	25.3	32.6	23.6	25.3	17.9	12.1
Receiving Food Stamps									
No	62.5***	68.1	89.2	34.7	27.7	65.1	61.1	73.9	68.0
Yes	37.5	31.9	10.8	65.3	72.3	34.9	38.9	26.1	32.0
Arrested since age 16									
No	85.0***	85.5	76.0	81.3	87.5	85.8	82.8	87.3	80.2
Yes	15.0	14.5	24.0	18.7	12.5	14.2	17.2	12.7	19.8
Arrested since age 16, by gender									
Women									
No	50.8***	55.1	43.7	38.6	51.1	40.6	61.1	54.5	40.5
Yes	2.8	4.4	6.0	2.7	3.3	1.9	5.6	0.7	2.0
Men									
No	34.2	30.4	32.3	42.7	36.4	45.2	21.7	32.8	39.7
Yes	12.2	10.1	18.0	16.0	9.2	12.3	11.6	11.9	17.8
Lived with both parents at age 14									
No	65.1***	73.9	55.1	73.3	78.3	55.7	59.6	49.3	45.7
Yes	34.9	26.1	44.9	26.7	21.7	44.3	40.4	50.7	54.3
Reason for leaving regular high school									
School-related	47.7***	39.1	53.9	46.7	50.0	51.9	42.9	54.5	45.7
Job-related	10.1	14.5	14.4	20.0	3.3	17.9	9.1	5.2	13.4
Other	42.2	46.4	31.7	33.3	46.7	30.2	48.0	40.3	40.9
Sample size	1,941	69	167	75	184	106	198	134	247

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup	All 13 Sites (%)	Sequential/In-House El Centro (Dallas) (%)	Sequential/In-House LA Job Corps (%)	Sequential/Brokered Allentown (Buffalo) (%)	Sequential/Brokered BSA (NYC) (%)	Sequential/Brokered CREC (Hartford) (%)
Own AFDC case or receiving General Assistance						
No	73.1***	83.2	68.8	51.1	76.1	70.7
Yes	26.9	16.8	31.2	48.9	23.9	29.3
Own AFDC case						
No	78.4***	83.2	70.6	68.1	82.1	73.7
Yes	21.6	16.8	29.4	31.9	17.9	26.3
Receiving Food Stamps						
No	62.5***	83.8	71.0	28.1	63.2	58.6
Yes	37.5	16.2	29.0	71.9	36.8	41.4
Arrested since age 16						
No	85.0***	88.8	90.5	86.7	88.9	82.8
Yes	15.0	11.2	9.5	13.3	11.1	17.2
Arrested since age 16, by gender						
Women						
No	50.8***	52.0	57.6	57.0	47.0	57.6
Yes	2.8	1.1	1.3	1.5	0.9	7.1
Men						
No	34.2	36.8	32.9	29.6	41.9	25.2
Yes	12.2	10.1	8.2	11.9	10.2	10.1
Lived with both parents at age 14						
No	65.1***	70.9	73.6	80.7	69.2	79.8
Yes	34.9	29.1	26.4	19.3	30.8	20.2
Reason for leaving regular high school						
School-related	47.7***	48.0	38.1	45.2	59.8	50.5
Job-related	10.1	8.4	13.9	5.2	3.4	7.1
Other	42.2	43.6	48.1	49.6	36.8	42.4
Sample size	1,941	179	231	135	117	99

(continued)

TABLE 2.1 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,941 sample members for whom there were 48 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The youths in the 48-month impact sample appear to have been more disadvantaged than the majority of youths served nationwide by JTPA Title IIA programs during the period JOBSTART was in operation. In the effort to serve those youths at risk of chronic unemployment, JOBSTART worked exclusively with school dropouts, a segment of the youth population that made up a relatively small proportion of JTPA Title IIA enrollees. For example, in the National JTPA Study, about 40 percent of the males and 50 percent of the females in the out-of-school youth sample had a high school diploma or GED. Even when the comparison of youths is limited to young school dropouts, it appears that JOBSTART reached a more disadvantaged population than did most JTPA Title IIA-funded programs.[19]

Nationwide, 80 percent of Job Corpsmembers were school dropouts in program year 1986, when the JOBSTART Demonstration was in operation, but their other characteristics suggest greater barriers to employment than the JOBSTART youths faced.[20] Job Corpsmembers tended to be younger than JOBSTART sample members: 42 percent were age 16 or under in 1986, compared to 29 percent in JOBSTART. Sixty-one percent read at the sixth-grade level or below at entry into the Job Corps, compared to 52 percent in JOBSTART.[21] On the other hand, a higher proportion of JOBSTART sample members were receiving public assistance and were members of minority groups than were Job Corpsmembers. The residential character of the Job Corps program also introduces another difference: All residential Corpsmembers are willing and able to live away from home, but an unknown – though probably large – portion of JOBSTART sample members would not be.[22]

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[19] Approximately 56 percent of the JOBSTART 48-month impact sample were receiving some form of public assistance at the time they entered the program (not shown in Table 2.1), compared to 39 percent of young dropouts served by JTPA Title IIA programs at the time and less than 35 percent of the youths in the National JTPA Study. Moreover, the proportion of the JOBSTART sample receiving AFDC at baseline (38 percent) was higher than that of young dropouts in other JTPA Title IIA programs (21 percent). This higher rate of welfare receipt partly reflects the fact that a greater proportion of the JOBSTART sample was female (53 percent), compared to the dropout group participating in other JTPA Title IIA programs (45 percent female). Also, minorities were much more heavily represented in JOBSTART than in JTPA Title IIA-funded services for young dropouts nationally. Hispanic dropouts constituted 44 percent of the JOBSTART sample but only 14 percent of JTPA Title IIA enrollees at the time, and JOBSTART served proportionally more black dropouts (46 percent) than did other JTPA Title IIA programs (34 percent). (See U.S. Department of Labor, Division of Performance Management and Evaluation, 1988.)

[20] U.S. Department of Labor, Employment and Training Administration, 1987.

[21] In JOBSTART, JTPA performance standards and practices led some JTPA Title IIA-funded sites to exclude youths with very low reading scores. The Job Corps sites in JOBSTART appeared to include a higher proportion of youths with very low reading scores than did other sites.

[22] The difficulties encountered in implementing an unsuccessful random assignment study of the residential versus nonresidential Job Corps programs illustrate the importance of this difference in characteristics. The study originally assumed that a substantial proportion of the Job Corps applicant pool would be indifferent

(continued...)

These comparisons suggest that JOBSTART sites did succeed in attracting disadvantaged young school dropouts, as intended in the demonstration. However, these young people were not among the most disadvantaged youths nationwide: only 5 percent of females and 26 percent of males reported at program intake that they had a criminal arrest record; most were not teenage parents; and about half had worked during the year before random assignment.[23] Forty-eight percent reported they had left school for school-related reasons such as poor grades, lack of interest, or discipline problems. In summary, the JOBSTART youths probably fell between the typical JTPA and Job Corps participant in baseline skill levels and job-readiness.

B. Site Differences in Sample Characteristics

When individuals with certain characteristics are concentrated in one or a few sites, the influence of their individual characteristics on program implementation and impacts are "confounded" with the influence of site characteristics. This problem is a virtual nonissue with regard to the proportion of experimentals and controls at the 13 sites: All sites had approximately equal proportions of the two groups. Among other characteristics, however, it is an extreme issue with regard to ethnicity: The proportion of black sample members at a site ranged from zero at the East Los Angeles Skills Center to over 97 percent at the Atlanta Job Corps, as shown in the individual site columns of Table 2.1. Thus, the influence of research status on implementation and impacts is almost independent of site, but the influence of ethnicity is much more confounded with the influence of site characteristics.[24] Therefore, while comparisons of experimental-control differences in post-program outcomes for the full sample may be confidently interpreted as resulting from experimentals' access to JOBSTART, there is not the same confidence about comparisons of participation or impacts for ethnic subgroups. These results could be heavily influenced by such site characteristics as program structure, labor market conditions, or other important factors completely

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[22](...continued)
as to whether they got into a residential or nonresidential program, and the study proposed to randomly assign members of this "indifferent" group to the two program types. This group turned out to be too small for the study to proceed.

[23] It is very likely that youths underreported past arrests, since they were asked about this at program intake and may have assumed that a positive response would lower their chances of getting into the program, or they may not have wanted to provide this information to staff, whom they did not yet know. In addition, only 106 men (12 percent of the male sample) reported being a father.

[24] Overall, most sample members were black (44 percent) or Hispanic (44 percent). In five sites, over two-thirds of the youths were black, while in three others, over two-thirds were Hispanic. The low overall proportion of white, non-Hispanic sample members (9 percent) was concentrated at the Phoenix Job Corps and, to a lesser extent, at Allentown in Buffalo, CET/San Jose, and EGOS in Denver.

external to JOBSTART. Most characteristics fall somewhere between "independent" and "confounded" in relation to site characteristics, and unless special techniques are used to remove associations between site and other characteristics, impact comparisons among many subgroups and site groupings may be misleading.

Fortunately, in view of the heavy emphasis in this report on comparing outcomes by gender, there is much less cause for concern in making comparisons of impacts for gender-defined subgroups than for those based on ethnicity. The validity of these comparisons is based on the fact that the impact sample was 46 percent male and 54 percent female overall, and men and women were distributed across sites much more evenly than were blacks or Hispanics.[25] A more refined analysis of gender, appearing throughout this report, splits the sample further, based on parenting status at random assignment. This breakdown creates categories that can signal differences in barriers to employment or willingness to sacrifice in order to obtain a steady source of earned income. Overall, half of the women lived with children of their own; among men, about one-eighth reported that they already were parents at baseline. The proportion of parents among women and men in the sample did vary among the sites, but the variation was much less than was the case for ethnicity.[26]

Site differences were large for several other subgroups of the research sample:

- **Sample members' amount of prior schooling varied among the sites more than gender or parenting status.** Large differences in baseline educational attainment are important to bear in mind when examining post-program GED attainment rates. Other factors aside, those who were closer to finishing high school at baseline were more likely to have received a GED during the follow-up period.

- **Employment during the year before random assignment varied among the sites even more than prior schooling.** Holding all other observed factors constant, not having worked recently may signal either greater barriers to employment or more interest in schooling than in employment.[27]

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[25] The proportion of males in a site ranged from a high of 59 percent at Chicago Commons to a low of 33 percent at EGOS in Denver. In addition to Chicago Commons, four other sites had male majorities: SER/Corpus Christi, the East Los Angeles Skills Center, BSA in New York City, and CET/San Jose.

[26] The proportion of men reporting fatherhood ranged from almost none at the Los Angeles Job Corps, BSA in New York City, and the East Los Angeles Skills Center to 19 percent at Chicago Commons. At Connelley in Pittsburgh, El Centro in Dallas, SER/Corpus Christi, EGOS in Denver, the Phoenix Job Corps, and Allentown in Buffalo, JOBSTART women were more likely to be custodial mothers, while at East Los Angeles Skills Center, BSA in New York, CET/San Jose, and CREC in Hartford, women who either had no children or were not living with them were in the majority among females.

[27] Overall, more men than women worked in the year prior to random assignment; for each gender subgroup, the proportion who had worked also varied among the sites. In eight sites, the majority of women had not worked in the year before random assignment, with the ratio of nonworking women to working women

(continued...)

- **Public assistance receipt varied greatly.** The percentage of a site's sample receiving public assistance may be a good indication of the relative levels of income and job-readiness of the young people there.[28]

In subsequent chapters of this report, program impacts for subgroups of the 48-month impact sample are presented. These subgroups are defined based on pre-random assignment (that is, pre-program) characteristics, and two types of analyses are used.[29] One type splits the entire sample into subgroups defined by a characteristic such as gender. This "split sample" subgroup analysis does not control in any way for other measured differences among the subgroups, such as in site or prior work experience. If a subgroup is concentrated in a few sites, as is the case for Hispanics, then the split-sample results may reflect site differences as much as subgroup differences. Since neither men nor women were concentrated in particular sites, the split-sample analysis presented for these subgroups in later chapters is appropriate. The second type of subgroup analysis – intended to address the problem of "confounded" influences on impacts discussed above – presents results that are statistically adjusted to account for measured pre-program differences among the subgroups in characteristics other than that used to define the subgroups. For example, it presents results for ethnic subgroups controlling for differences in measured characteristics other than ethnicity, such as site and age. This analysis does control for site differences and thus can be used for subgroups that are relatively concentrated in a few sites.

C. Sample Differences for Key Site Groupings

In view of the current interest among policymakers in the influence of a site's delivery system for education and training – whether it is concurrent, sequential/in-house, or sequential/brokered – Table 2.2 collapses the 13 site columns of Table 2.1 into three columns, one for each type of delivery system.[30] The column labeled "p" is a measure of the likelihood that the observed differences in

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[27](...continued)
above 2:1 at the Phoenix Job Corps and Allentown in Buffalo. However, Connelley in Pittsburgh and CREC in Hartford were notable exceptions, with substantial majorities of women having had prior-year work experience. Men's prior employment profiles by site were the opposite, with ratios of employed to nonemployed as high as 4:1 at SER/Corpus Christi, CREC in Hartford, and the Atlanta Job Corps. Only at Los Angeles Job Corps and BSA in New York City did male nonworkers outnumber workers, with the former's ratio of nonworkers to workers exceeding 2:1.

[28] The average baseline reading level on entering the program also varied among the sites. This is not discussed in detail because comparable initial test scores are not available for all the sites, as outlined earlier.

[29] Defining subgroups based on pre-random assignment characteristics is necessary to maintain the legitimacy of the comparisons of experimentals and controls. For example, those who had not worked in the year prior to random assignment were just as likely to be randomly assigned to the experimental group as to the control group, making comparisons of experimentals and controls with this characteristic valid.

[30] The eight concurrent sites in Table 2.1 are collapsed into the "concurrent" column of Table 2.2; El Centro in Dallas and the Los Angeles Job Corps into the "sequential/in-house" column; and Allentown in
(continued...)

TABLE 2.2

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY PROGRAM STRUCTURE

Characteristic and Subgroup	Sample Size	Concurrent (%)	Sequential/ In-House (%)	Sequential/ Brokered (%)	All Categories (%)	p^a
Gender						
Women	1,041	51.8	56.3	56.7	53.6	0.125
Men	900	48.2	43.7	43.3	46.4	
Ethnicity						
White, non-Hispanic	172	10.6	4.9	7.7	8.9***	0.000
Black, non-Hispanic	860	33.2	57.6	66.1	44.3	
Hispanic	847	53.6	30.5	25.6	43.6	
Other	62	2.6	7.1	0.6	3.2	
Ethnicity, by gender						
Women						
White, non-Hispanic	97	5.8	2.9	4.8	5.0***	0.000
Black, non-Hispanic	467	18.5	30.5	35.3	24.1	
Hispanic	451	26.6	19.5	16.2	23.2	
Other	26	0.9	3.4	0.3	1.3	
Men						
White, non-Hispanic	75	4.8	2.0	2.8	3.9	
Black, non-Hispanic	393	14.7	27.1	30.8	20.2	
Hispanic	396	26.9	11.0	9.4	20.4	
Other	36	1.7	3.7	0.3	1.9	
Parental status						
Women living with own child(ren)						
No	533	26.9	26.8	29.9	27.5**	0.027
Yes	508	24.8	29.5	26.8	26.2	
Men who have own child(ren)						
No	785	41.4	41.0	36.8	40.4	
Yes	115	6.9	2.7	6.6	5.9	
Employed within past year						
No	914	40.5	62.2	51.6	47.1***	0.000
Yes	1,027	59.5	37.8	48.4	52.9	
Prior employment, by gender						
Women employed within past year						
No	583	26.7	37.3	32.8	30.0***	0.000
Yes	458	25.1	19.0	23.9	23.6	
Men employed within past year						
No	331	13.8	24.9	18.8	17.1	
Yes	569	34.4	18.8	24.5	29.3	
Sample size	1,941	1,180	410	351		

(continued)

TABLE 2.2 (continued)

Characteristic and Subgroup	Sample Size	Concurrent (%)	Sequential/ In-House (%)	Sequential/ Brokered (%)	All Categories (%)	p^a
Left school in grade 11 or 12						
No	1,140	60.3	52.9	60.1	58.7**	0.027
Yes	801	39.7	47.1	39.9	41.3	
Received occupational training within past year						
No	1,615	81.4	90.2	80.9	83.2***	0.000
Yes	326	18.6	9.8	19.1	16.8	
Age						
16-19	1,425	70.9	79.0	75.2	73.4***	0.004
20 or 21	516	29.1	21.0	24.8	26.6	
Marital status						
Ever married	184	11.8	7.6	4.0	9.5***	0.000
Never married	1,757	88.2	92.4	96.0	90.5	
Living in own household or with boy/girlfriend						
No	1,575	80.0	88.5	76.4	81.1***	0.000
Yes	366	20.0	11.5	23.6	18.9	
Own AFDC case or receiving General Assistance						
No	1,418	74.7	75.1	65.0	73.1***	0.001
Yes	523	25.3	24.9	35.0	26.9	
Own AFDC case						
No	1,522	80.4	76.1	74.4	78.4**	0.023
Yes	419	19.6	23.9	25.6	21.6	
Receiving Food Stamps						
No	1,214	61.9	76.6	48.4	62.5***	0.000
Yes	727	38.1	23.4	51.6	37.5	
Arrested since age 16						
No	1,649	82.9	89.8	86.3	85.0***	0.003
Yes	292	17.1	10.2	13.7	15.0	
Arrested since age 16, by gender						
Women						
No	986	48.4	55.2	53.8	50.8**	0.019
Yes	55	3.4	1.2	2.9	2.8	
Men						
No	663	34.5	34.6	32.5	34.2	
Yes	237	13.7	9.0	10.8	12.2	
Sample size	1,941	1,180	410	351		

(continued)

TABLE 2.2 (continued)

Characteristic and Subgroup	Sample Size	Concurrent (%)	Sequential/ In-House (%)	Sequential/ Brokered (%)	All Categories (%)	p[a]
Lived with both parents at age 14						
No	1,264	59.2	72.4	76.6	65.1***	0.000
Yes	677	40.8	27.6	23.4	34.9	
Reason for leaving regular high school						
School-related	925	48.3	42.4	51.6	47.7***	0.002
Job-related	197	11.2	11.5	5.1	10.1	
Other	819	40.5	46.1	43.3	42.2	
Site						
Concurrent						
Atlanta Job Corps	69	5.8	0.0	0.0	3.6***	0.000
CET/San Jose	167	14.2	0.0	0.0	8.6	
Chicago Commons	75	6.4	0.0	0.0	3.9	
Connelley (Pittsburgh)	184	15.6	0.0	0.0	9.5	
East LA Skills Center	106	9.0	0.0	0.0	5.5	
EGOS (Denver)	198	16.8	0.0	0.0	10.2	
Phoenix Job Corps	134	11.4	0.0	0.0	6.9	
SER/Corpus Christi	247	20.9	0.0	0.0	12.7	
Sequential/in-house						
El Centro (Dallas)	179	0.0	43.7	0.0	9.2	
LA Job Corps	231	0.0	56.3	0.0	11.9	
Sequential/brokered						
Allentown (Buffalo)	135	0.0	0.0	38.5	7.0	
BSA (NYC)	117	0.0	0.0	33.3	6.0	
CREC (Hartford)	99	0.0	0.0	28.2	5.1	
Sample size	1,941	1,180	410	351		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,941 sample members for whom there were 48 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The column labeled "p" is the statistical significance level of differences among groups in distributions of characteristics: that is, p is the probability that observed proportions in each subgroup differ from one column to another only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

characteristics among the sites occurred because of random error; the lower the p-value, the more likely it is that the observed differences are "real" rather than caused because the sample drawn was unusual. Sample members at sequential/in-house sites were younger, less likely to have acknowledged fatherhood on the enrollment form, less likely to have worked during the prior year, more likely to have completed the 10th grade, and more likely to have previously attended an occupational training program than were sample members in other sites. Sample members in concurrent sites were older, much less likely to be black, much more likely to be Hispanic, more likely to have worked during the prior year, less likely never to have been married, less likely to be receiving AFDC, and more likely to have lived with two parents at age 14 than were sample members in other sites.[31] One possible generalization is that those recruited at concurrent sites had more prior work experience, while those at sequential sites had more formal education and training. These differences may be related to the sites' programs, with concurrent sites emphasizing the job connection more and sequential sites beginning with education.

D. Gender Differences in Baseline Characteristics

An important question in evaluating JOBSTART is whether participation and program impacts vary by gender. A first step toward understanding gender differences is to examine the other characteristics of the various gender-defined subgroups. Table 2.3 shows that men and women in the impact sample were similar in many characteristics, including age, ethnicity, educational attainment, and initial reading level. However, men were more likely to have had recent work experience and vocational training and to have been arrested since age 16. They were less likely to have been married, to be a parent, and to be receiving public assistance.

Most of these differences between men and women are due mainly to differences between custodial mothers and other members of the sample. Custodial mothers were least likely to have worked in the year before random assignment, most likely to have lived on their own, most likely to have received AFDC and Food Stamps, and least likely to have lived with both parents at age 14. Because of these clear differences of custodial mothers in baseline characteristics, the gender-based

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[30](...continued)
Buffalo, BSA in New York City, and CREC in Hartford into the "sequential/brokered" column. Averaging data for sites in broad categories destroys much of the observed site variation – particularly regarding ethnicity, receipt of welfare and Food Stamps, parenting status, amount of schooling, and prior-year employment.

[31] There may also be unobserved differences. For example, youths attracted to sequential/brokered programs run by community-based educational institutions may have been more interested in passing the GED examination than youths at concurrent sites run by training agencies.

TABLE 2.3

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY GENDER AND PARENTAL STATUS

		Women		Men			
Characteristic and Subgroup	Sample Size	Living with Own Child(ren) (%)	All Others (%)	Do Not Have Own Child(ren) (%)	Have Own Child(ren) (%)	All Categories (%)	p^a
Ethnicity							
White, non-Hispanic	172	8.1	10.5	9.2	2.6	8.9***	0.000
Black, non-Hispanic	860	52.6	37.5	41.9	55.7	44.3	
Hispanic	847	38.8	47.7	44.5	40.9	43.6	
Other	62	0.6	4.3	4.5	0.9	3.2	
Ethnicity, by gender							
Women							
White, non-Hispanic	97	8.1	10.5	0.0	0.0	5.0***	0.000
Black, non-Hispanic	467	52.6	37.5	0.0	0.0	24.1	
Hispanic	451	38.8	47.7	0.0	0.0	23.2	
Other	26	0.6	4.3	0.0	0.0	1.3	
Men							
White, non-Hispanic	75	0.0	0.0	9.2	2.6	3.9	
Black, non-Hispanic	393	0.0	0.0	41.9	55.7	20.2	
Hispanic	396	0.0	0.0	44.5	40.9	20.4	
Other	36	0.0	0.0	4.5	0.9	1.9	
Employed within past year							
No	914	61.8	50.5	38.3	26.1	47.1***	0.000
Yes	1,027	38.2	49.5	61.7	73.9	52.9	
Prior employment, by gender							
Women employed within past year							
No	583	61.8	50.5	0.0	0.0	30.0***	0.000
Yes	458	38.2	49.5	0.0	0.0	23.6	
Men employed within past year							
No	331	0.0	0.0	38.3	26.1	17.1	
Yes	569	0.0	0.0	61.7	73.9	29.3	
Sample size	1,941	508	533	785	115		

(continued)

TABLE 2.3 (continued)

Characteristic and Subgroup	Sample Size	Women		Men		All Categories (%)	p^a
		Living with Own Child(ren) (%)	All Others (%)	Do Not Have Own Child(ren) (%)	Have Own Child(ren) (%)		
Left school in grade 11 or 12							
No	1,140	61.6	59.1	56.8	57.4	58.7	0.385
Yes	801	38.4	40.9	43.2	42.6	41.3	
Received occupational training within past year							
No	1,615	87.8	85.2	79.7	77.4	83.2***	0.000
Yes	326	12.2	14.8	20.3	22.6	16.8	
Age							
16-19	1,425	61.2	84.8	77.5	47.0	73.4***	0.000
20 or 21	516	38.8	15.2	22.5	53.0	26.6	
Marital status							
Ever married	184	19.5	5.8	2.9	27.0	9.5***	0.000
Never married	1,757	80.5	94.2	97.1	73.0	90.5	
Living in own household or with boy/girlfriend							
No	1,575	53.9	87.6	94.5	80.0	81.1***	0.000
Yes	366	46.1	12.4	5.5	20.0	18.9	
Own AFDC case or receiving General Assistance							
No	1,418	37.2	84.2	87.5	80.9	73.1***	0.000
Yes	523	62.8	15.8	12.5	19.1	26.9	
Own AFDC case							
No	1,522	39.4	90.1	94.1	89.6	78.4***	0.000
Yes	419	60.6	9.9	5.9	10.4	21.6	
Sample size	1,941	508	533	785	115		

(continued)

TABLE 2.3 (continued)

		Women		Men			
Characteristic and Subgroup	Sample Size	Living with Own Child(ren) (%)	All Others (%)	Do Not Have Own Child(ren) (%)	Have Own Child(ren) (%)	All Categories (%)	p^a
Receiving Food Stamps							
No	1,214	43.7	67.7	71.5	60.9	62.5***	0.000
Yes	727	56.3	32.3	28.5	39.1	37.5	
Arrested since age 16							
No	1,649	95.7	93.8	74.1	70.4	85.0***	0.000
Yes	292	4.3	6.2	25.9	29.6	15.0	
Arrested since age 16, by gender							
Women							
No	986	95.7	93.8	0.0	0.0	50.8***	0.000
Yes	55	4.3	6.2	0.0	0.0	2.8	
Men							
No	633	0.0	0.0	74.1	70.4	34.2	
Yes	237	0.0	0.0	25.9	29.6	12.2	
Lived with both parents at age 14							
No	1,264	73.0	64.9	59.6	68.7	65.1***	0.000
Yes	677	27.0	35.1	40.4	31.3	34.9	
Reason for leaving regular high school							
School-related	925	21.5	56.7	59.0	44.3	47.7***	0.000
Job-related	197	2.6	7.3	15.3	21.7	10.1	
Other	819	76.0	36.0	25.7	33.9	42.2	
Sample size	1,941	508	533	785	115		

(continued)

TABLE 2.3 (continued)

Characteristic and Subgroup	Sample Size	Women		Men			
		Living with Own Child(ren) (%)	All Others (%)	Do Not Have Own Child(ren) (%)	Have Own Child(ren) (%)	All Categories (%)	p^a
Site							
Concurrent							
Atlanta Job Corps	69	31.9	27.5	36.2	4.3	3.6***	0.000
CET/San Jose	167	5.4	44.3	45.5	4.8	8.6	
Chicago Commons	75	21.3	20.0	40.0	18.7	3.9	
Connelley (Pittsburgh)	184	32.1	22.3	35.9	9.8	9.5	
East LA Skills Center	106	12.3	30.2	55.7	1.9	5.5	
EGOS (Denver)	198	36.9	29.8	30.3	3.0	10.2	
Phoenix Job Corps	134	30.6	24.6	39.6	5.2	6.9	
SER/Corpus Christi	247	24.3	18.2	48.2	9.3	12.7	
Sequential/in-house							
El Centro (Dallas)	179	31.3	21.8	42.5	4.5	9.2	
LA Job Corps	231	28.1	30.7	39.8	1.3	11.9	
Sequential/brokered							
Allentown (Buffalo)	135	32.6	25.9	29.6	11.9	7.0	
BSA (NYC)	117	18.8	29.1	51.3	0.9	6.0	
CREC (Hartford)	99	28.3	36.4	29.3	6.1	5.1	
Sample size	1,941	508	533	785	115		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,941 sample members for whom there were 48 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The column labeled "p" is the statistical significance level of differences among groups in distributions of characteristics: that is, p is the probability that observed proportions in each subgroup differ from one column to another only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

subgroup analysis in this report includes *three* subgroups: men, custodial mothers, and all other women. In most of the analysis, all men are usually grouped together because of the small number reporting that they were parents at baseline.

CHAPTER 3

JOBSTART SERVICES AND PROGRAM PARTICIPATION

The JOBSTART model required sites to operate basic education and occupational skills training classes that would be interesting and accessible, effective in improving the skills of young people, and of relatively long duration. It also required young people to take advantage of these opportunities. Historically, education and training programs have had problems retaining young, economically disadvantaged dropouts (or even high school graduates).[1] Thus, a key question for the evaluation is whether youths offered JOBSTART services did actually participate in lengthy, intensive services. As will become apparent from the findings in this chapter, JOBSTART was not the same program for all youths in the experimental group.

This chapter looks at the JOBSTART experience from three perspectives.[2] First, it briefly summarizes the nature of program services (highlighting key aspects of site variation) and reports youths' subjective reactions to the services.[3] Second, it describes the participation patterns of youths who were active in the JOBSTART Demonstration and compares those patterns to other programs for young school dropouts. The analysis shows that participation was, in general, longer and more substantial than in most other JTPA Title IIA-funded activities for young dropouts, and that it was roughly comparable to participation in intensive programs such as the nonresidential Job Corps and the National Supported Work Demonstration (generally referred to simply as Supported Work).

Third, the chapter analyzes the extent to which participation varied among different groups of youths and types of sites. This analysis finds that average participation hours were similar for many groups: males and females, various ethnic groups, older and younger participants, youths with relatively higher and lower levels of reading skills, and recipients and nonrecipients of public assistance. But behind these averages there is substantial variation, with over one-third of the sample receiving a very weak version of the JOBSTART program and about one-fifth participating more than 700 hours.

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[1] U.S. Department of Education, National Center for Education Statistics, 1989; Public/Private Ventures, 1988; Kelly, 1987.

[2] The chapter summarizes and updates information presented in Chapters 2 and 4 through 8 of Auspos et al., 1989. See that report for more details.

[3] These reactions were captured in the initial follow-up survey, which was conducted 12 months after random assignment, and in focus groups with participants. This section presents information on the JOBSTART components in specific sites.

The discussion of youths' experiences reinforces two basic themes of this report. First, the variation in the details of the programs highlights the diversity of JOBSTART experiences among the sites within the general framework of the JOBSTART guidelines. And second, the experience of the sites shows that the basic program model can be implemented in a variety of administrative and labor market settings and using different basic program structures, though there were clearly stronger and weaker programs among the sites in the demonstration.

I. The Nature of the JOBSTART Services

Basic education, occupational training, support services, and job placement assistance were available to participants in each site. To operate JOBSTART, two of the six community-based organizations (SER/Corpus Christi and Chicago Commons) added education to their regular service offerings, and three of the others (Allentown in Buffalo, BSA in New York City, and CREC in Hartford) developed or strengthened relationships with outside training programs so that they could serve as brokers, arranging training elsewhere for JOBSTART participants. The one community college (El Centro in Dallas) and three adult vocational schools (Connelley in Pittsburgh, the East Los Angeles Skills Center, and EGOS in Denver) had previously offered education and training but had to strengthen support services and job placement assistance. The three Job Corps Centers (in Atlanta, Los Angeles, and Phoenix) already had all four kinds of services in place. CET/San Jose already operated a program of integrated training and education, with support services and job placement assistance.

Table 3.1, which groups the sites by whether they operated concurrent, sequential/in-house, or sequential/brokered programs, describes the entry and exit rules, availability of separate classes for youths, expected duration of occupational training, and scheduled hours per day in each site.[4] In some sites, participants could enter courses at any time (open entry) and leave them when they had achieved a certain competency level (open exit), while in others they had to adhere to a fixed cycle, with entry on specified dates and exit after a set period of time. Some sites held classes for youths only, while others mixed youths and adults. Sites also varied in their expected duration of training, daily scheduling, and support services.

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[4] This grouping of sites was chosen because, as discussed later, participation rates by component, participation hours, and program emphasis differed among the three types.

TABLE 3.1

CHARACTERISTICS OF JOBSTART ACTIVITIES, BY SITE

Site	Fixed Cycle or Open Entry and Exit	Separate Classes for Youths	Expected Duration of Occupational Training	Scheduled Hours per Day			
				Education[a]	Training	Other Activities	Total
Concurrent							
Atlanta Job Corps	Open entry and exit	Yes	1 year maximum[b]	Individualized, usually 2 hours	Individualized, usually 2.5 hours at start, more in subsequent weeks	Usually 2 hours in life skills and avocational activities at start, less in subsequent weeks[c,d]	6.5 hours
CET/San Jose	Open entry and exit	In education only	600-1,000 hours during 23-37 weeks	2 hours, may vary	4.5 hours, may vary	None	6.5 hours
Chicago Commons	Fixed cycle	In education only	500-1,380 hours during 22-42 weeks	1-2 hours, 3-5 days per week	4.5-7 hours, depending on course	None	6.5-8 hours
Connelley (Pittsburgh)	Fixed cycle with semesters	Sometimes in education	700-1,000 hours	2 hours	4 hours	1 hour of counseling and other support services in school year 1986-87[c]	6 hours in school year 1985-86, 7 hours in school year 1986-87
East LA Skills Center	Open entry and exit	No	600-840 hours during 20-28 weeks	2 hours, may vary	4 hours, may vary	None	6 hours
EGOS (Denver)	Open entry and exit with semesters	In education only	600-1,000 hours	2 hours, may vary	4 hours, may vary	None[c]	6 hours
Phoenix Job Corps	Open entry and exit	Yes	1 year maximum[b]	Individualized, usually 2 hours	Individualized, usually 2.5 hours at start, more in subsequent weeks	Usually 2 hours in life skills and avocational activities at start, less in subsequent weeks[c,d]	6.5 hours
SER/Corpus Christi	Fixed cycle	Yes	500-660 hours during 22-23 weeks	2.5 hours for first 12-16 weeks[e]	3.5 hours for first 12-16 weeks, then 6 hours	None	6 hours

(continued)

TABLE 3.1 (continued)

Site	Fixed Cycle or Open Entry and Exit	Separate Classes for Youths	Expected Duration of Occupational Training	Scheduled Hours per Day			
				Education[a]	Training	Other Activities	Total
Sequential/in-house							
El Centro (Dallas)	Open entry and exit	In education only	720 hours during 24 weeks	3-4 hours	6 hours	2-3 hours in life skills activities during education phase[c,d]	6 hours
LA Job Corps	Open entry and exit	Yes	1 year maximum[b]	3 hours for first 10-12 weeks, then individualized	6 hours, may vary	3 hours in life skills or avocational activities during education phase[c,d]	6 hours
Sequential/brokered							
Allentown (Buffalo)	Open entry and exit for education, varied in training	In education only	Varied by training provider	3 hours	Varied by training provider	3 hours in life skills activities during education phase[d]	6 hours during education phase
BSA (NYC)	Open entry and exit for education, varied in training	In education only	Varied by training provider	3 hours, 4 days per week	Varied by training provider	3 hours in life skills activities during education phase, 4 days per week[d]	6 hours during education phase, 4 days per week
CREC (Hartford)	Open entry and exit for education, varied in training	No	Varied by training provider	3 hours	Varied by training provider	None[c]	3 hours during education phase

SOURCE: Adapted from Auspos et al., 1989.

NOTES: [a] Education hours refer to time spent in a basic education or GED-preparation class and do not include education provided as part of an occupational training course.

[b] Job Corps Centers offered a maximum of two years of training, but JOBSTART participants were supposed to be enrolled in courses that could be completed in one year.

[c] Some participants had paid or unpaid work experience positions for limited periods.

[d] Life skills classes typically provided instruction in work behaviors, goal-setting, personal budgeting, health, and interpersonal relations. Avocational activities included physical education and driver education.

[e] Additional hours were available on an individualized basis after the course ended.

A. Basic Education

The education component typically consisted of individualized instruction, which allowed students to move at their own pace learning reading, mathematics, and other subjects needed to pass the GED examination. Mostly they worked on their own, doing workbook exercises or, less commonly, using computer-assisted instruction. In sites offering education and training concurrently, participants usually attended two hours of education classes and four hours of vocational training a day. In sites operating a sequential program, participants generally attended three hours a day of basic skills classes during the education phase, with the remaining three hours a day devoted to life skills classes.

The payment provisions of the contracts between service providers and funding agencies (especially local SDAs) were an important source of variation in the emphasis of the education component. In four sites (Connelley in Pittsburgh, EGOS in Denver, El Centro in Dallas, and SER/Corpus Christi), payment for education services was based on students passing the GED examination. This led these sites to make GED certification an important short-term goal of the program and to emphasize the skills tested on the GED examination in their education component. Other sites – CET/San Jose, the East Los Angeles Skills Center, and especially Chicago Commons – saw GED attainment as a long-term goal and did not stress it in their JOBSTART programs, focusing more on improving basic skills as an aid to vocational training and job placement.

The actual curricula and instructional materials were not specified by the JOBSTART guidelines. The three Job Corps sites used the standard Job Corps materials (workbooks, textbooks, and audiovisual materials), though two centers (Atlanta and Phoenix) also had supplementary computer-assisted instruction. The three sequential/brokered sites used the Comprehensive Competencies Program (CCP) developed by U.S. Basic Skills Investment Corporation. CCP is an instructional management system integrating textbooks, workbooks, computer software, audiovisual materials, and progress tests. In the seven other sites, teachers developed their own instructional materials using a variety of sources, such as GED preparation courses and reading and mathematics textbooks that use the "mastery learning" approach, which focuses on the step-by-step acquisition of specific competencies. In four of these sites, staff supplemented pencil-and-paper exercises with computer-assisted instruction.[5]

Teachers in most sites felt that the individualized, self-paced instruction provided a better learning environment than participants had typically found in high school. The competency-based courses allowed the youths to see themselves making incremental progress as they advanced toward what was, for many, a remote goal of mastering basic skills and receiving a GED. Most students preferred this instructional approach because they felt that it made them active participants in the process of learning and allowed them to master one topic before beginning another. In the follow-up survey, about three-fourths of JOBSTART participants found self-paced instruction "very helpful," while virtually no one found it "not helpful at all."

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[5] See Auspos et al., 1989, for the details of these programs.

Yet students also valued interaction with instructors, as much for the personal attention and motivation it provided as for instruction in specific skills. About 75 percent of JOBSTART participants rated support from teachers and fellow students in the education component "very helpful."

Despite the overall favorable assessment, three concerns emerged. First, with a few important exceptions, the basic education and skills training activities operated separately, with little integration of material. As discussed in Chapter 1, only at CET/San Jose, and to a lesser extent Chicago Commons, were basic skills and occupational training instruction truly integrated. Though several other sites did attempt to coordinate the two activities to a limited extent (creating a distribution of sites rather than two clear-cut categories), these sites fell short of the integration observed at CET/San Jose and Chicago Commons. Second, some instructors thought the curriculum should include more material on critical thinking and general knowledge, in contrast to the functional literacy and mathematics emphasis of many integrated programs. Third, some instructors said that students with very poor skills or low motivation found the work boring and, as a remedy, suggested more group activities. One site, El Centro in Dallas, shifted to this approach, relying more heavily than other sites on class exercises and lectures.

B. Occupational Skills Training

The choices of occupational training available to participants varied among the sites. Participants at large vocational schools could choose courses in more than 20 occupational areas. The Job Corps Centers and larger community-based organizations also offered a wide range of vocational training. In contrast, SER/Corpus Christi, which provided training in-house, offered only a few courses.

In theory, youths in sequential/brokered sites could choose courses from a variety of local agencies. However, in practice, some courses were unavailable to them because they could not satisfy entrance requirements, or other difficulties prevented them from gaining entry.[6] As discussed later in this chapter, the resulting low rate of participation in training in sequential/brokered sites was the major operational issue concerning the training component.

As a group, JOBSTART participants were enrolled in training for a broad range of occupations – clerical and service jobs, machine trades, benchwork occupations, and structural work such as welding. Occupational choices for men and women followed traditional patterns, as shown in Table 3.2, with about three-fourths of the female participants training for clerical jobs.[7]

Using categories employed by the U.S. General Accounting Office (GAO) in a recent analysis of JTPA Title IIA adult training, MDRC classified the JOBSTART training provided to participants as leading to jobs requiring low or low/moderate skills (slightly less than one-fourth of participants), moderate skills (about one-half of participants), and higher skills (about one-fourth of participants).[8] This distribution of skills ratings for training occupations was similar to what GAO found for JTPA Title IIA adult programs. This was unexpected, since JOBSTART participants faced more barriers to employment than did the typical JTPA Title IIA adult client.

One argument for sequential programs is that the upfront education allows participants to enter more advanced training. In the JOBSTART Demonstration, however, this did not appear to occur. In terms of the GAO categories, the jobs for which youths trained in sequential sites did not appear to require higher skills than those in concurrent sites.

C. Support Services to Facilitate Participation

All sites provided basic support services such as assistance with transportation and child care, which helped participants to attend the program, as shown in Table 3.3. All sites provided bus passes or small allowances to cover the costs of commuting to the program. JOBSTART counselor/coordinators placed a high priority on adequate child care arrangements. In most sites, staff referred JOBSTART participants to other agencies for child care, with the expenses being covered by JTPA or the Work Incentive (WIN) Program. The Atlanta Job Corps, two CBOs (SER/Corpus Christi and CET/San Jose), and one adult school (Connelley in Pittsburgh) had on-site day care facilities, but staff

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[6] As mentioned in Chapter 1, the JTPA performance standards created an incentive for SDAs to emphasize lower-cost, short-term programs. Some SDAs in study sites were reluctant to provide a single individual with both education and training, and many JTPA Title IIA-funded service providers operated under performance-based contracts linking payment to placement in a job. Both practices hindered the efforts of JOBSTART youths in sequential/brokered sites to find a training agency willing to accept them.

[7] This table, taken from an earlier report on JOBSTART (Auspos et al., 1989), is based on a similar, but slightly smaller, sample than that used for this report.

[8] See U.S. General Accounting Office, 1988, for the definitions of categories of training. The percentage distribution reported above for JOBSTART was calculated in Auspos et al., 1989, based on a slightly different sample of participants than that used in this report.

TABLE 3.2

PERCENTAGE DISTRIBUTION OF OCCUPATIONS FOR PARTICIPANTS IN JOBSTART TRAINING, BY GENDER

Training Category[a]	Men (%)	Women (%)	Men and Women (%)
Clerical and sales occupations			
Stenography, typing, filing, and related occupations	5.0	51.3	29.2
Computing and account-recording	7.0	20.3	13.9
Production and stock clerks, and related occupations	0.3	0.0	0.1
Information and message distribution	1.4	0.0	0.7
Miscellaneous clerical	0.0	0.5	0.3
Sales and consumable commodities	0.8	1.0	0.9
Total	14.6	73.1	45.1
Service occupations			
Food and beverage preparation and services	3.1	4.6	3.9
Miscellaneous personal services	0.6	11.5	6.3
Building and related services	8.4	2.1	5.1
Total	12.0	18.2	15.3
Machine trades occupations			
Metal machining	5.0	0.8	2.8
Mechanics and machinery repair	22.4	1.3	11.4
Printing	0.6	1.3	0.9
Wood machining	0.6	0.5	0.5
Total	28.6	3.8	15.7
Benchwork occupations			
Assembly and repair of electrical equipment	11.5	1.3	6.2
Painting, decorating, and related occupations	0.8	0.3	0.5
Fabrication and repair of plastics, synthetics, rubber, and related products	2.2	0.3	1.2
Fabrication and repair of textile, leather, and related products	1.7	0.3	0.9
Total	16.2	2.1	8.8
Structural work occupations			
Metal fabricating	9.8	0.5	5.0
Welders, cutters, and related occupations	0.8	0.0	0.4
Electrical assembling, installing, and repairing	5.9	0.5	3.1
Painting, plastering, waterproofing, cementing, and related occupations	1.7	0.0	0.8
Construction	8.4	1.3	4.7
Total	26.6	2.3	13.9
Miscellaneous occupations			
Transportation	0.0	0.3	0.1
Graphic art work	2.0	0.3	1.1
Total	2.0	0.5	1.2
All training categories	100.0	100.0	100.0
Sample size	357	390	747

(continued)

TABLE 3.2 (continued)

SOURCE: Adapted from Auspos et al., 1989. The categorization of occupations is derived from U.S. Department of Labor, 1977.

NOTES: Calculations for this table used data for all experimentals who were active for at least one hour in a JOBSTART training component within 12 months of random assignment and responded to the 12-month follow-up survey.

Distributions may not total 100.0 percent because of rounding.

When totaled, individual category percentages may not equal the general category percentage because of rounding.

Tests of statistical significance were not performed.

[a] Individuals participating in more than one training category were included in the category in which they attended the most hours.

TABLE 3.3

BASIC SUPPORT SERVICES AVAILABLE IN JOBSTART, BY SITE

Site	Needs-Based Payments	Transportation	Child Care	Other	Incentive Payments
Job Corps Centers					
Atlanta Job Corps	Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months	Bus passes	On-site	Free meals; clothing allowance of $75 in first month, $50 in third month, $96 in sixth and tenth months, $51 in twelfth month; on-site medical and dental care	Merit raises could increase basic allowance to $100 per month after 6 months; $75 per month was placed in escrow for enrollees who stayed 6 months, which increased to $100 per month after 6 months; $150 bonus in tenth month
LA Job Corps	Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months	Bus passes	By referral	Free meals; clothing allowance of $75 in first month, $50 in third month, $96 in sixth and tenth months, $51 in twelfth month; on-site medical and dental care	Merit raises could increase basic allowance to $100 per month after 6 months; $75 per month was placed in escrow for enrollees who stayed 6 months, which increased to $100 per month after 6 months; $150 bonus in tenth month
Phoenix Job Corps	Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months	Bus passes	By referral	Free meals; clothing allowance of $75 in first month, $50 in third month, $96 in sixth and tenth months, $51 in twelfth month; on-site medical and dental care	Merit raises could increase basic allowance to $100 per month after 6 months; $75 per month was placed in escrow for enrollees who stayed 6 months, which increased to $100 per month after 6 months; $150 bonus in tenth month
Schools					
Connelley (Pittsburgh)	$5 per day[a]	$2 per day or bus passes[a]	On-site and by referral	$50 one-time clothing grant	$50 for passing GED;[b] $50 for each month of perfect attendance; quarterly payment of $50 for "A" average, $25 for "B" average, $10 for "C" average

(continued)

TABLE 3.3 (continued)

Site	Needs-Based Payments	Transportation	Child Care	Other	Incentive Payments
East LA Skills Center	None	Bus passes, gasoline vouchers	By referral	Emergency funds, lunch money during a brief period	None
EGOS (Denver)	None	Bus passes, gasoline vouchers	By referral	Lunch money during a brief period	None
El Centro (Dallas)	$5 per day	Bus passes	By referral	Emergency rent funds	$5 per week for perfect attendance
Community-based organizations					
Allentown (Buffalo)	$1 per hour if on AFDC, otherwise $2 per hour, during education and training	Included in needs-based payment	By referral	None	None
BSA (NYC)	$23-$30 per week during education,[c] $30 per week during JTPA training	Included in needs-based payment; tokens available otherwise	By referral, $15 per week for expenses	Free breakfasts	$5 for weekly academic progress; $5 for perfect weekly attendance[d]
CET/San Jose	$1 per hour, for farmworkers only	Bus passes for farmworkers and others who demonstrated need	On-site and by referral	Weekly food bank to provide free groceries	None
Chicago Commons	$6 per day	Included in needs-based payment	By referral	None	None
CREC (Hartford)	None	Bus passes	By referral	None	None
SER/Corpus Christi	$8 per day	Included in needs-based payment	On-site for children over 18 months and by referral	None	$20 for each grade-level gain in reading; $20 for passing GED pre-test; $40 for passing GED test; $45 for "A" average throughout occupational training, $25 for "B" average

SOURCE: Adapted from Auspos et al., 1989.

NOTES: [a] At intervals, this site combined transportation and needs-based payments into one $7 per day payment.
[b] Available during 1986-87 school year.
[c] During October 1986-August 1987.
[d] Available after October 1987.

reported that students frequently preferred to make their own arrangements in their own neighborhoods.

Many sites also found ways to provide small payments to meet other costs of participating, though the Job Corps Centers were consistently able to provide more support than the other sites. Ten of the 13 sites were able to provide some type of small needs-based payment, while nine of the sites provided on-site meals or food to take home, or special allowances for clothing or to meet rent emergencies. Seven of the sites (including all three Job Corps Centers) provided some form of incentive payments to participants who reached milestones in the program. The Job Corps Centers also provided on-site medical and dental care.

In addition, to increase participants' motivation and commitment to the program, site staff used a variety of strategies: personal counseling, peer support, time management training, and group recreational activities. Finally, staff at most sites provided training in life skills – covering topics such as health, personal finances, and workplace routines – to help the young people function more responsibly and productively in a variety of roles and situations. Six of the sites (the three Job Corps programs, El Centro in Dallas, Allentown in Buffalo, and BSA in New York City) incorporated two to three hours of formal life skills classes into the regular program day.[9] The remaining seven sites did not focus as systematically on life skills, instead incorporating these topics into the training curriculum, counseling or group discussion sessions, or occasional lectures.

Youths cited personal attention from staff as a crucial aid in helping them move toward self-sufficiency. While agencies that traditionally served disadvantaged youths typically offered these support services from the beginning of the demonstration, a number of sites accustomed to serving adults increased this type of activity as their programs evolved.

Clearly, the support services and other activities available at the Job Corps Centers surpassed those at the other sites in both breadth and intensity. To a large extent, the JOBSTART participants in these sites were able to partake of the full array of Job Corps activities, including recreational, health, and food services. However, Allentown in Buffalo, Connelley in Pittsburgh, and SER/Corpus Christi also offered high levels of these services.

D. Job Placement Services

Sites were required to assist youths in finding training-related employment, but this phase of the program typically received less attention than others. Nearly all the sites did provide instruction about employers' expectations as well as job search techniques. About one-half of the sites arranged paid or unpaid part-time work experience positions for some participants during the program. Approximately one-fourth of a sample of participants worked at some point – in program-arranged or self-initiated jobs – while they were active in the program. Those in the sample who were employed worked an average of about 50 percent of the weeks they were in the JOBSTART program and were employed an average of about 30 hours per week during the weeks they worked. During the months they worked, their hours of classes in JOBSTART were lower than were those of nonworking participants.

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[9] The life skills curricula in these sites were oriented around daily living, with units on health education, substance abuse prevention, sexuality and family planning, personal finances, civics, communication skills, goal-setting and planning, and improving self-esteem.

Efforts to find participants permanent employment typically began near the end of training, with instructor contacts serving as an important source of information about job openings. Since many youths left the program without reaching this stage, it is not surprising that only about one-fourth of participants reported that program staff referred them to a job or told them about openings.

Job placement assistance was especially strong at CET/San Jose, Chicago Commons, and the Job Corps program in Phoenix. In all these sites, instruction in proper work behavior, employer expectations, and job search techniques began while students were still in training; placement specialists provided leads and assistance in finding a job; and CET/San Jose and Chicago Commons had especially strong ties to local employers.

Job placement assistance was noticeably weaker at Allentown in Buffalo, BSA in New York City, and CREC in Hartford (the three sequential/brokered sites), and at the Atlanta Job Corps, the East Los Angeles Skills Center, EGOS in Denver, and SER/Corpus Christi (among the concurrent sites). At the sequential/brokered sites, job placement was intended to be the responsibility of the training agency, but most participants were never active in that component, so only informal assistance was available from the JOBSTART agency. Those concurrent sites with weak job placement typically lacked any or sufficient in-house job development specialists, were larger agencies with no special emphasis on placing JOBSTART youths, or contracted out job placement to another organization that did not see the JOBSTART youths as a high-priority group.

E. Scheduling, Daily Service Mix, and Planned Program Duration

Sites also varied in the way they scheduled classes and the expected duration of their programs. The demonstration sites scheduled JOBSTART classes in three basic ways. The majority of sites that operated both the education and training components themselves scheduled the classes on an open-entry/open-exit basis.[10] This means that participants could enter the program at any time, progress through the material at their own pace, and complete the course whenever they reached the specified competency levels. The duration of training was open-ended, but sites anticipated that participants would typically be able to complete the prescribed training curriculum in many fields in approximately 600 to 800 hours. Individuals who needed additional time to complete competencies could stay longer, however.

In a second program variation, some concurrent sites operated JOBSTART as a series of "fixed cycles," meaning that all participants started and completed training together on specified dates and the maximum length of training was prescribed.[11] In a third variation, the three sequential/brokered sites operated the education component on an open-entry/open-exit schedule, but the training schedule was determined by the variety of training organizations at which JOBSTART participants were enrolled.

Sites also showed great variety in the number of hours scheduled for activities each day. The usual schedule ranged from a low of three hours per day at CREC in Hartford to seven to eight hours per day in some courses at Chicago Commons. A typical day can be described in terms of three basic models:

- **Concurrent sites that were CBOs or schools.** Students typically had six hours of classes per day, five days a week. In general, two hours were spent in education classes, with training classes scheduled for the remaining four hours.

- **Concurrent sites that were Job Corps Centers.** These sites had six and a half class hours per day. Schedules were highly individualized and changed frequently, but commonly included two hours of education, two and a half hours of vocational training, and two hours devoted to life skills, health education, or avocational activities such as sports.

- **Sequential sites.** These also scheduled a six-hour day during the education phase, but the daily distribution of activities was quite different. Typically, three hours were spent in education classes and another three hours were spent in life skills training. The training schedules were set by the training providers at the brokered sites, but typically involved five to six hours of classes per day. Training classes ran for six hours a day at the sequential/in-house sites.

The duration of the occupational training component also varied among the sites, ranging from 22 to 23 weeks at SER/Corpus Christi to a year at the Job Corps sites. Even within a site there could be significant variation among the different training options. At Chicago Commons, for example, scheduled training ranged from 500 hours in industrial inspection to 1,380 hours in packaging-machine repair.

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[10] These sites included concurrent sites (the Atlanta Job Corps, CET/San Jose, the East Los Angeles Skills Center, and the Phoenix Jobs Corps) and sequential/in-house sites (El Centro in Dallas and the Los Angeles Job Corps).

[11] These sites included Chicago Commons, Connelley in Pittsburgh, EGOS in Denver, and SER/Corpus Christi.

This diversity in scheduled daily hours and program duration meant that the planned participation hours for youths varied greatly across the sites, with the greatest variation showing in the training component. At SER/Corpus Christi, a participant completing education and training in about six months, as planned, would have had no more than 660 hours of occupational training. In contrast, one training course at Chicago Commons totaled nearly 1,400 hours, and a sequential program such as the one operated by the Los Angeles Job Corps could last for as long as a year.[12]

F. Summary of Program Implementation by Site

Table 3.4 rates the implementation of the four central JOBSTART components in each site. (See Auspos et al., 1989, for the details behind these ratings.) The information in Table 3.4 and the material already presented in this section suggest that the four key components of the JOBSTART program were implemented most successfully at CET/San Jose,[13] Chicago Commons, Connelley in Pittsburgh, El Centro in Dallas, the Los Angeles Job Corps, and the Phoenix Job Corps, and least successfully at the Atlanta Job Corps, BSA in New York City, CREC in Hartford, and EGOS in Denver. To summarize the ratings by component:

- **Education.** Most sites that chose to operate a separate education component were able to offer an activity that met the JOBSTART guidelines. The two sites with noticeably weak education activities were the Atlanta Job Corps (where unclear objectives for education and staff turnover hampered implementation) and CREC in Hartford (where computer facilities were underutilized and attendance was a serious problem).

- **Training.** The training component showed the most variation – primarily because in sequential/brokered sites most youths never participated in training. In addition, the limited training offerings, less experienced staff, and older equipment of SER/Corpus Christi (reflecting the common problems of a community-based organization) hampered its ability to implement the training component.

- **Support services.** Although the Job Corps Centers did offer substantially more services than other sites, all programs were able to provide the limited types of

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[12] Job Corps Centers offered a maximum of two years of training, but JOBSTART participants were only to be enrolled in courses that could be completed in one year.

[13] CET/San Jose provided most of its basic education services within training activities, so this overall characterization reflects a judgment about the other three components and the way in which education was incorporated into the training component.

TABLE 3.4

RATINGS OF THE IMPLEMENTATION OF JOBSTART COMPONENTS, BY SITE

Site	Education	Training	Support Services	Job Placement
Concurrent				
Atlanta Job Corps	Low	Medium	High	Low
CET/San Jose	No rating[a]	High	Medium	High
Chicago Commons	Medium	High	Medium	High
Connelley (Pittsburgh)	High	High	High	Medium
East LA Skills Center	Medium	Medium	Medium	Low
EGOS (Denver)	Medium	Medium	Medium	Low
Phoenix Job Corps	Medium	High	High	High
SER/Corpus Christi	High	Low	High	Low
Sequential/in-house				
El Centro (Dallas)	High	Medium	Medium	Medium
LA Job Corps	Medium	Medium	High	Medium
Sequential/brokered				
Allentown (Buffalo)	High	Low	High	Low
BSA (NYC)	Medium	Low	Medium	Low
CREC (Hartford)	Low	Low	Medium	Low

SOURCE: MDRC operations staff.

NOTES: See Auspos et al., 1989, for details of the implementation of components in each site.

[a] In this site, a separate rating of the education component was inappropriate because education and training were more integrated than in other sites and staff strongly emphasized training over passing the GED examination.

support services called for in the JOBSTART guidelines: assistance in arranging and/or financing child care and transportation to and from coursework. Allentown in Buffalo, Connelley in Pittsburgh, and SER/Corpus Christi, in addition to the three Job Corps Centers, provided a noticeably longer list of services, including better needs-based payments, life skills training and counseling, and a method of identifying service needs and making referrals of youths to other agencies providing the required services.

- **Job placement assistance.** Most sites offered job placement assistance that fell short of the JOBSTART guidelines, either because many youths never received the service (especially in sequential/brokered sites) or too few staff with a specialty in job search assistance were assigned to work with the JOBSTART youths. As mentioned earlier, job placement assistance was especially strong at CET/San Jose, Chicago Commons, and the Phoenix Job Corps.

II. The Intensity of JOBSTART Participation

Participation in JOBSTART among experimentals was measured by participation rates in each activity, hours of participation in each activity, and overall length of stay. Table 3.5 shows these summary measures for all experimentals in the impact sample:[14]

- **Participation rates.** Nearly 90 percent of all experimentals in the impact sample participated in JOBSTART to some extent. Eighty-six percent of all experimentals (and nearly all of those who were active in JOBSTART) attended basic skills education classes, 67 percent participated in training, and 40 percent participated in other activities, which were optional for sites.

- **Average participation hours.** Average hours were 125 in education, 238 in training, and 37 in other activities, for a total of 400 hours.[15]

- **Distribution of participation hours.** Forty-one percent of all experimentals spent fewer than 201 hours in all JOBSTART activities; 25 percent spent 201 to 500 hours; and 33 percent spent more than 500 hours.

- **Length of stay.** The average length of stay was 6.6 months, with the median length being slightly less, 5.9 months; 77 percent of experimentals were active for 3 months or more, while 53 percent stayed in the program for 6 months or more. Length of stay was measured from the time of random assignment through the last

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[14] As discussed in Chapter 2, the findings in this report are slightly different from those presented in prior reports because of small differences in the samples used for the analysis.

[15] These averages and those cited in the next paragraph include the 11 percent of the sample with zero hours of participation in JOBSTART activities.

TABLE 3.5

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS

Activity Measure	Outcome
Percent participating in	
Any activity	88.8
Education	85.5
Training	66.5
Education and training	65.2
Other activities	39.7
Average hours in	
Education	124.5
Training	238.2
Education and training	362.8
Other activities	36.9
All activities	399.9
Percentage distribution of hours in education and training	
None	11.9
Up to 200	35.0
201 to 500	21.6
501 to 700	15.9
701 or more	15.6
Total	100.0
Percentage distribution of hours in all activities	
None	11.2
Up to 200	30.0
201 to 500	25.4
501 to 700	15.1
701 or more	18.3
Total	100.0
Length of stay (months)	
Average	6.6
Median	5.9
Months receiving services	
Average	5.9
Median	5.0
Percent still participating in month	
3	76.7
6	52.9
9	29.5
12	15.4
15	8.7
18	4.4
19 or later	3.3
Sample size	988

SOURCE: MDRC calculations from JOBSTART enrollment form and MIS data.

NOTES: Calculations for this table used data for all 988 experimentals for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

month that included any hours of participation.[16] Fifteen percent of the experimental sample were still active in the program in the twelfth month after random assignment, while 9 percent were still active in the fifteenth month.

These findings show that JOBSTART succeeded in engaging more than half of the youths in the experimental group in the program and its activities, but that for about 40 percent of them participation was quite low and JOBSTART did not constitute an intensive or lengthy program. Because of this wide range of participation levels, with some participants receiving very few hours of education and training, the average total hours for the sample as a whole is the equivalent of less than three and one-half months of regular attendance for six hours per day. Most people in the sample did not participate long enough to get a GED or complete a training course.

To place these results in context, JOBSTART participation may be compared to reported participation in other programs for young, disadvantaged school dropouts. Length of participation is a simple measure that permits comparison with three types of youth programs: JTPA Title IIA programs for young dropouts, the Job Corps, and Supported Work.[17] JTPA Title IIA typically provides relatively short-term activities, while the Job Corps and Supported Work have been among the most intensive employment and training programs for disadvantaged youths. In these comparisons, either the average or median length of participation was used, depending on the available data.

Overall, JOBSTART participants stayed in the program considerably longer than did young dropouts in JTPA Title IIA activities, as shown in Table 3.6. During program year 1986, when the demonstration was in operation, the median length of participation for all young dropouts in JTPA

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[16] As indicated in Table 3.5, the average number of months during which participants actually received services is somewhat lower – 5.9 months. The "length of stay" in the program is higher because the period of participation could include months of inactivity if a person stopped attending classes and then returned to the program within the 12-month follow-up period. However, this does not appear to have been a common pattern. Among a sample of participants, about 85 percent did not have any months of inactivity within the period they were counted as active, and among those with inactivity, the average period of inactivity was about two months. Youths who attended JOBSTART were counted as participating for the entire month in which they were randomly assigned and all months in which they showed any JOBSTART hours. The measure might have overestimated the length of participation somewhat when a youth was randomly assigned late in a month or ended participation early in a month.

[17] For information on the Job Corps, see Richardson and Burghardt, 1985, and U.S. Department of Labor, Employment and Training Administration, 1987. On Supported Work, see Maynard, 1980. For JTPA Title IIA, see U.S. Department of Labor, 1988. Hours of attendance were not reported for all the programs, hence this comparison uses length of stay, for which the data were available.

TABLE 3.6

PARTICIPATION AND LENGTH OF STAY FOR YOUNG DROPOUTS IN JTPA TITLE IIA PROGRAMS, BY ACTIVITY

Activity	Percentage Distribution of Youths in JTPA	Median Length of Stay (Months)
Classroom activities		
Basic education	22.8	3.71
Occupational skills training	15.6	3.98
Combined basic education and occupational skills training[a]	4.6	6.97
Total	42.9[b]	3.97
On-the-job training	12.2	3.14
Job search assistance	15.3	0.81
Work experience	7.8	3.67
Other services	21.8	3.59
Any activity	100.0[c]	3.40

SOURCE: U.S. Department of Labor, Division of Performance Management and Evaluation, 1988.

NOTES: This table includes data for young dropouts served under JTPA Title IIA during program year 1986.

[a] JTPA data (as recorded by the U.S. Department of Labor, Division of Performance Management and Evaluation, 1988) combined basic education and occupational skills training under the label CT-Other.

[b] The distribution may not total 42.9 percent because of rounding.

[c] The distribution may not total 100.0 percent because of rounding.

Title IIA programs was 3.4 months compared to 6 months for JOBSTART.[18] JOBSTART's median length of participation exceeded that of young dropouts in all JTPA components except one. The exception was a program combining basic education and occupational skills training, a mix similar to JOBSTART's, which had a median length of 7 months but was offered to only 5 percent of all young dropouts in JTPA Title IIA activities. For JOBSTART participants active in both education and skills training, the median length of stay in the program was also approximately 7 months. These findings support the conclusion that JOBSTART achieved its goal of operating a program more intensive than that typically offered in JTPA Title IIA programs for young dropouts. JOBSTART participation was also longer than that observed for out-of-school youths in the National JTPA Study, where less than half were still enrolled in JTPA three months after random assignment.[19]

JOBSTART's average length of participation was less than that of the Job Corps or Supported Work. During program year 1986, the average stay in the Job Corps was 6.9 months, compared to JOBSTART's average of 6.6 months.[20] Supported Work was an experimental program of *paid* work experience under conditions of gradually increasing responsibility on the job, close supervision, and work in association with a crew of peers. It operated from 1975 to 1979 and included young school dropouts, many with a criminal record, as one of its target groups. While precise comparisons are impossible, the length of participation in Supported Work appears to have been slightly longer.[21] The average length of participation in Supported Work was 6.7 months (compared to 6.6 in JOBSTART) and the median was approximately 6 months (the same as in JOBSTART), and 25 percent of Supported Work participants were still active in the program at 12 months after random assignment, as opposed to 15 percent for JOBSTART.

In summary, while only approximate comparisons can be made, it appears that JOBSTART achieved its goal of providing young school dropouts with more intensive education and training than is usual within the JTPA system. The data also suggest that JOBSTART offered a duration of activity almost as long as that of the Job Corps or Supported Work, which operated through special agencies and had the sole mission of providing services to very disadvantaged individuals. However, for the 41 percent of the JOBSTART sample who had very few hours of activity or did not participate, the treatment was unlikely to be intense and lengthy enough to move them above the threshold of skills needed to secure significantly better jobs than they could before the program.

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[18] The average length of participation in JTPA Title IIA programs was not available from published sources. The figure for JTPA Title IIA includes only persons who actually participated, while the JOBSTART figure includes the 11 percent of the sample made up of nonparticipants with zero months of activity.

[19] Bloom et al., 1993.

[20] The median for the Job Corps was not available.

[21] The JOBSTART measure of length of participation included some periods of inactivity in the midst of participation, while the Supported Work measure factored these out. As discussed above, however, this problem does not appear to have been serious in the JOBSTART data.

III. Moving Behind the Aggregate Participation Measures

Aggregate measures tell only part of the story, however. Table 3.5 makes clear that JOBSTART was not the same experience for all youths: 41 percent participated for 200 or fewer total hours, while more than 18 percent exceeded 700 hours, the required offering under the demonstration (200 hours of education and 500 hours of training). Clear differences in average participation also existed among the sites, as discussed later in this chapter. Understanding the sources of these variations in participation is the first step in developing ways to improve the design and implementation of the program.

The following analysis begins with subgroups of JOBSTART experimentals defined based on pre-random assignment characteristics. It shows that while there were differences among subgroups, they did not seem to account for all the variation in participation. This implies that factors such as unmeasured differences among youths, local employment opportunities, and program characteristics associated with particular sites may also have affected participation. The key finding on program characteristics is that youths in sites operating sequential/brokered programs tended to have lower rates of participation in occupational skills training, although they tended to receive more intensive instruction in basic skills.

A. Differences in Participation Among Subgroups

Although JOBSTART youths all satisfied the program's eligibility requirements, when they entered the program they varied in gender, age, marital and parental status, criminal records, and educational attainment, among other characteristics. Research and operational experience suggest that these types of factors can influence participation in programs.[22]

Among JOBSTART experimentals, two subgroups are of special concern: males (who have often been hard to recruit and retain in education and training programs) and young mothers (a group at risk of long-term welfare receipt). As Table 3.7 shows, average total hours and other measures of participation were similar for all males and females, although a higher percentage of females were active in the twelfth month after random assignment. There were some differences,

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[22] See, for example, Public/Private Ventures, 1988, and Mathematica Policy Research, 1985.

TABLE 3.7

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY, BY GENDER AND PARENTAL STATUS

		Women			
Activity Measure	Men	Living with Own Child(ren)	Not Living with Own Child(ren)[a]	All Women	Men and Women
Percent participating in					
Any activity	88.4	89.9	88.3	89.1	88.8
Education	84.8	86.8	85.5	86.1	85.5
Training	66.7	68.9	64.0	66.3	66.5
Education and training	64.7	67.7	63.6	65.6	65.2
Other activities	35.7	42.8	43.1	43.0	39.7**
Average hours in					
Education	118.3	115.2	142.9	129.7**	124.5
Training	249.4	226.0	231.7	229.0	238.2
Education and training	367.7	341.3	374.5	358.7	362.8
Other activities	31.5	37.3	45.3	41.5	36.9***
All activities	399.1	379.4	419.8	400.6	399.9
Percentage distribution of hours in education and training					
None	12.3	10.9	12.4	11.7	11.9
Up to 200	31.9	42.0	33.6	37.6	35.0
201 to 500	23.2	17.1	23.0	20.5	21.6
501 to 700	18.1	14.8	13.4	14.2	15.9
701 or more	14.5	15.2	17.7	16.0	15.6
Total					
Percentage distribution of hours in all activities					
None	11.6	10.1	11.7	11.0	11.2
Up to 200	27.7	35.0	29.0	32.0	30.0
201 to 500	26.6	23.7	25.1	24.6	25.4
501 to 700	16.7	14.4	13.1	13.8	15.1
701 or more	17.4	16.7	21.2	18.6	18.3
Total					
Average length of stay (months)	6.2	6.8	6.9	6.9	6.6*
Average months receiving services	5.7	6.1	6.1	6.1	5.9
Percent still participating in month					
3	75.7	78.2	77.0	77.6	76.7
6	52.2	52.9	54.1	53.5	52.9
9	27.2	31.1	31.4	31.3	29.5
12	10.9	18.3	19.8	19.1	15.4***
15	6.9	9.3	11.0	10.2	8.7*
18	2.9	6.2	4.9	5.6	4.4**
19 or later	2.5	3.9	4.2	4.1	3.3
Sample size	448	257	283	540	988

SOURCE: MDRC calculations from JOBSTART enrollment form and MIS data.

NOTES: Calculations for this table used data for all 988 experimentals for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Among all women, the distributions compared were those for women who were living with their own child(ren) and those for women not living with their own child(ren), including those who were childless, at the time of random assignment. An F-statistic was used to test the hypothesis of equal column means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] Includes women who did not have children.

however, for females living with their children compared to other women: Mothers averaged somewhat fewer hours of participation, and a higher percentage received fewer than 200 hours of services.

Table 3.8 presents average total hours of participation in JOBSTART for other subgroups. Although past research and experience suggest that the characteristics listed in the table might affect participation, many of the comparisons do not show significant differences in average hours for the subgroups under review. Youths who had been arrested since age 16 (most of whom were males) participated for significantly fewer hours than those who had not,[23] but other subgroups did not show statistically significant differences in hours.

B. Differences in Participation Among Sites

Hours of participation in the sites in the demonstration varied considerably, as shown in Table 3.9. Average total hours ranged from a high of 607 for experimentals at the Los Angeles Job Corps to a low of 171 at CREC in Hartford, a spread of 436 hours. Another important aspect of variation was the percentage of experimentals still participating in JOBSTART at 12 months after random assignment. This proportion varied from a low of zero percent at Chicago Commons and SER/Corpus Christi to a high of 41 percent at Allentown. The proportion still active at 18 months ranged from zero percent to 16 percent at the Los Angeles Job Corps. Thus, the 48 months of follow-up do not represent the same length of *post-program* follow-up at all sites.

As noted earlier, this variation could have had several possible sources, such as characteristics of the youths, local employment opportunities, and program characteristics.[24] With only 13 sites in the demonstration, it is very difficult to isolate the effects on participation of the many differences among programs. If, for example, the sites with the most support services were also Job Corps sites and also operated a youths-only program, it would be impossible to separate out the effects of these individual factors on participation hours. Further, the demonstration was not designed to address this type of question with the same rigor provided for comparisons of experimentals and controls.

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[23] The mix of activities did differ by initial reading score. Those testing in the low group averaged slightly more hours in education, and had somewhat fewer hours in training, than those in the highest group.

[24] Differences in the way random assignment interacted with site recruitment efforts, and also in attendance reporting, led to variations in participation rates as well. At CET/San Jose, for example, services were not available for the first part of the sample for up to one month. Furthermore, as discussed in Chapter 2, attendance at a multi-day assessment of occupational training interests was not included in reported hours. As a result, 36 percent of the experimentals at that site had no reported hours in program services. At other sites, the gap between random assignment and reported program start-up was shorter and participation rates were higher.

TABLE 3.8

AVERAGE TOTAL PARTICIPATION HOURS, BY SELECTED CHARACTERISTICS OF EXPERIMENTALS AT THE TIME OF RANDOM ASSIGNMENT

Characteristic and Subgroup	Average Total Hours	Number of Experimentals
Age		
16-19	393.4	724
20 or 21	417.9	264
Ethnicity[a]		
White, non-Hispanic	364.1	82
Black, non-Hispanic	389.0	440
Hispanic	387.0	439
School grade at time of dropout		
Grade 10 or below	407.8	570
Grade 11 or 12	389.1	418
Reading grade level		
1-6	359.1	282
7-8	368.3	190
9 or above	327.9	20
Gender		
Women	400.6	540
Men	399.1	448
Marital status		
Ever married	378.7	97
Never married	402.2	891
Parental status		
Women living with own child(ren)	373.5	257
Women not living with own child(ren)[b]	420.0	283
AFDC benefits received		
None	385.0	607
Own AFDC case	434.5	200
Household AFDC case	411.7	181
Received occupational training within past year		
No	403.9	833
Yes	378.3	155
Reason for leaving regular high school		
School-related	410.4	489
Job-related	407.4	91
Other	385.8	408
Criminal record		
No arrest since age 16	415.4	837
Arrested since age 16	314.1***	151
Sample size		988

(continued)

TABLE 3.8 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and MIS data.

NOTES: Calculations for this table used data for all 988 experimentals for whom there were 48 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

An F-statistic was used to test the hypothesis of equal means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] The sample also included 27 experimentals who were members of other ethnic groups.

[b] Includes women who did not have children.

TABLE 3.9

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS, BY SITE

		Concurrent							
Activity Measure	All 13 Sites	Atlanta Job Corps	CET/ San Jose	Chicago Commons	Connelley (Pittsburgh)	East LA Skills Center	EGOS (Denver)	Phoenix Job Corps	SER/ Corpus Christi
Percent participating in									
Education	86.7***	81.8	46.4	80.0	95.6	88.0	94.2	82.9	94.4
Training	66.5***	78.8	58.3	92.5	98.9	88.0	80.6	82.9	96.0
Education and training	65.2***	78.8	45.9	80.0	95.6	88.0	80.6	82.9	94.4
Other activities	39.7***	84.8	0.0	0.0	0.0	0.0	0.0	78.6	0.0
Average hours in									
Education	124.4***	102.4	25.6	69.1	98.9	75.9	127.5	160.6	118.1
Training	238.2***	177.4	309.1	353.0	438.8	293.6	142.2	201.5	282.3
Education and training	362.1***	279.8	334.7	422.0	537.6	369.5	269.8	362.1	400.4
Other activities	36.4***	53.7	0.0	0.0	0.0	0.0	0.0	58.0[a]	0.0
All activities	398.4***	333.5	334.7	422.0	537.6	369.5	269.8	420.0	400.4
Percentage distribution of hours in education and training									
None	12.0***	18.2	35.7	7.5	1.1	12.0	5.8	17.1	4.0
Up to 200	35.0	36.4	19.0	35.0	23.1	32.0	48.5	32.9	17.6
201 to 500	21.7	21.2	13.1	17.5	23.1	18.0	27.2	20.0	36.8
501 to 700	15.9	18.2	9.5	12.5	19.8	18.0	10.7	11.4	41.6
701 or more	15.3	6.1	22.6	27.5	33.0	20.0	7.8	18.6	0.0
Percentage distribution of hours in all activities									
None	11.3***	15.2	35.7	7.5	1.1	12.0	5.8	14.3	4.0
Up to 200	30.1	27.3	19.0	35.0	23.1	32.0	48.5	31.4	17.6
201 to 500	25.5	30.3	13.1	17.5	23.1	18.0	27.2	21.4	36.8
501 to 700	15.1	15.2	9.5	12.5	19.8	18.0	10.7	11.4	41.6
701 or more	18.1	12.1	22.6	27.5	33.0	20.0	7.8	21.4	0.0
Average length of stay (months)	6.6***	5.8	4.1	4.3	9.5	5.7	7.1	6.3	5.0
Average months receiving services	5.9***	5.3	3.6	4.1	8.5	5.3	6.4	6.0	4.8
Percent still participating in month									
3	76.7***	66.7	53.6	62.5	95.6	70.0	82.5	77.1	84.8
6	52.9***	39.4	39.3	42.5	72.5	54.0	58.3	48.6	58.4
9	29.5***	24.2	17.9	7.5	61.5	30.0	34.0	30.0	0.0
12	15.4***	15.2	6.0	0.0	22.0	8.0	19.4	18.6	0.0
15	8.7***	6.1	0.0	0.0	14.3	0.0	6.8	5.7	0.0
18	4.4***	3.0	0.0	0.0	8.8	0.0	1.9	1.4	0.0
19 or later	3.3***	3.0	0.0	0.0	4.4	0.0	0.0	1.4	0.0
Sample size	988	33	84	40	91	50	103	70	125

(continued)

TABLE 3.9 (continued)

Activity Measure	All 13 Sites	Sequential/In-House		Sequential/Brokered		
		El Centro (Dallas)	LA Job Corps	Allentown (Buffalo)	BSA (NYC)	CREC (Hartford)
Percent participating in						
Education	86.7***	98.9	78.4	100.0	73.3	86.5
Training	66.5***	47.3	52.6	31.0	23.3	17.3
Education and training	65.2***	47.3	52.6	31.0	23.3	17.3
Other activities	39.7***	98.9	81.0	97.2	75.0	17.3
Average hours in						
Education	124.4***	146.8	145.5	238.0	148.6	124.4
Training	238.2***	178.6	354.8	104.5	62.6	35.4
Education and training	362.1***	325.4	600.4	342.5	211.2	159.8
Other activities	36.4***	82.6	104.9	84.6	70.9	10.9
All activities	398.4***	408.0	607.0	427.2	282.2	170.7
Percentage distribution of hours in education and training						
None	12.0***	1.1	21.6	0.0	26.7	13.5
Up to 200	35.0	51.6	30.2	50.7	35.0	61.5
201 to 500	21.7	17.2	14.7	19.7	23.3	17.3
501 to 700	15.9	17.2	6.0	15.5	6.7	3.8
701 or more	15.3	12.9	27.6	14.1	8.3	3.8
Percentage distribution of hours in all activities						
None	11.3***	1.1	19.0	0.0	25.0	13.5
Up to 200	30.1	32.3	19.0	38.0	26.7	59.6
201 to 500	25.5	35.5	21.6	26.8	30.0	17.3
501 to 700	15.1	9.7	7.8	14.1	3.3	5.8
701 or more	18.1	21.5	32.8	21.1	15.0	3.8
Average length of stay (months)	6.6***	6.0	7.8	10.7	5.6	6.3
Average months receiving services	5.9***	5.4	7.0	8.8	4.8	5.6
Percent still participating in month						
3	76.7***	84.9	73.3	91.5	61.7	63.5
6	52.9***	47.3	46.6	78.9	40.0	42.3
9	29.5***	26.9	37.1	53.5	26.7	30.8
12	15.4***	8.6	23.3	40.8	16.7	21.2
15	8.7***	1.1	20.7	31.0	11.7	11.5
18	4.4***	1.1	15.5	11.3	3.3	3.8
19 or later	3.3***	1.1	12.9	11.3	1.7	3.8
Sample size	988	93	116	71	60	52

(continued)

TABLE 3.9 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and MIS data.

NOTES: Calculations for this table used data for all 988 experimentals for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. An F-statistic was used to test the hypothesis of equal column means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] The Phoenix Job Corps did not report hours spent by participants in life skills or avocational activities.

Applicants were randomly assigned to the experimental or control group, but there was no random assignment to various types of sites, and within each labor market there was usually only one site. This means that the power of the random assignment research design applies to differences between experimentals and controls (at a site or in the aggregate) and for differences among subgroups (as defined by pre-random assignment characteristics). Other types of comparisons, such as between types of sites, are inherently less reliable, and the strength of conclusions depends on the consistency of results across sites.[25]

Analysis presented in a previous report found that differences in experimentals' characteristics among the sites explained only a small part of the differences in average total hours.[26] The analysis also found that labor market conditions affected participation: Sites with better employment opportunities, other things being equal, had lower average participation hours.[27]

The strongest influence on participation in JOBSTART appeared to be program structure: whether a site was concurrent, sequential/in-house, or sequential/brokered. Participation rates by component, participation hours, and percentage of time in education or training all differed among the three types of sites, as detailed in Table 3.9 and summarized in Table 3.10.[28] Four conclusions about program structure can be drawn:

- **Average hours of participation varied by type of site.** Experimentals in sequential/in-house sites had the highest average participation hours, while those in sequential/brokered sites had by far the lowest because of very low average hours in training.

- **The mix of education, training, and other activities varied by type of site.** The concurrent sites, other than two Job Corps sites, did not offer the optional "other activities" and emphasized occupational training; as a result, average training hours for experimentals amounted to 71 percent of average total hours.[29] The sequential/brokered sites emphasized education and other nontraining services, all of which were provided in-house. They had the highest average hours in education; training hours were only about 22 percent of average total hours. The

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[25] See Chapter 5 for a more detailed discussion of the difficulty of making cross-site comparisons.

[26] See Auspos et al., 1989.

[27] This could have been because those participating in JOBSTART found a job more easily and left the program after fewer hours. Alternatively, sites in labor markets with low unemployment may recruit youths who have more unmeasured barriers to employment, are harder to work with in a program, and end up with fewer hours of participation.

[28] As discussed earlier, eight sites provided concurrent basic education and occupational skills training ("concurrent" sites); two provided education followed by training ("sequential/in-house" sites); and three provided education and then referred participants to other agencies for training ("sequential/brokered" sites).

[29] The percentage of average total hours is calculated by dividing average training hours by average total hours. Percentages that follow are similarly calculated.

TABLE 3.10

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY, BY PROGRAM STRUCTURE

Activity Measure	Concurrent	Sequential/ In-House	Sequential/ Brokered	All Catagories
Percent participating in				
Education	84.2	87.6	87.4	85.5
Training	85.1	50.2	24.6	66.5***
Education and training	82.9	50.2	24.6	65.2***
Other activities	13.9	89.0	67.2	39.7***
Average hours in				
Education	101.0	146.1	176.4	124.5***
Training	276.1	276.4	71.1	238.2***
Education and training	377.2	422.5	247.6	362.8***
Other activities	9.8	95.0	59.2	36.9***
All activities	387.0	518.5	306.8	399.9***
Percentage distribution of hours in education and training				
None	11.6	12.4	12.6	11.9***
Up to 200	29.2	39.7	48.6	35.0
201 to 500	24.0	15.8	20.2	21.6
501 to 700	19.6	11.0	9.3	15.9
701 or more	15.6	21.1	9.3	15.6
Total				
Percentage distribution of hours in all activities				
None	11.1	11.0	12.0	11.2***
Up to 200	28.5	24.9	40.4	30.0
201 to 500	24.7	27.8	25.1	25.4
501 to 700	19.5	8.6	8.2	15.1
701 or more	16.3	27.8	14.2	18.3
Total				
Average length of stay (months)	6.1	7.0	7.7	6.6**
Average months receiving services	5.6	6.6	6.2	5.9**
Percent still participating in month				
3	77.0	78.5	73.8	76.7
6	54.2	46.9	55.7	52.9
9	25.7	32.5	38.3	29.5***
12	11.2	16.7	27.3	15.4***
15	4.4	12.0	19.1	8.7***
18	2.0	9.1	6.6	4.4***
19 or later	1.0	7.7	6.0	3.3***
Sample size	569	209	183	988

SOURCE: MDRC calculations from JOBSTART enrollment form and MIS data.

NOTES: Calculations for this table used data for all 988 experimentals for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. An F-statistic was used to test the hypothesis of equal column means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

sequential/in-house sites had the highest total average hours: hours in education and other nontraining activities approximating those of the sequential/brokered sites, and hours in training like those of the concurrent sites.

- **Sequential/brokered sites had difficulty moving participants from education to training.** Only 25 percent of participants at sequential/brokered sites made the transition to occupational training, although those who made the transition did receive substantial training. This low rate of participation in training occurred owing to the difficulty of linking participants with other organizations, in part owing to the nature of typical JTPA Title IIA contracts. Possibly, it also arose because participants in these sites (which were primarily basic education organizations) were more interested in receiving a GED than occupational training.

- **These relationships do not appear to have been the result of measured differences in participant characteristics or local employment opportunities.** Even after adjustments for measured differences in participant characteristics and local employment opportunities, these patterns of participation among sites with different program structures still appear.[30]

While these three site categories do clarify patterns of participation, the sites within each category were clearly not identical. Among the concurrent sites, EGOS in Denver stood out with especially low hours – possibly because of its very large size, which could have left the JOBSTART youths feeling isolated and disconnected from the program. CREC in Hartford, among the sequential/brokered sites, had very low hours because it scheduled only three hours of education per day and very few experimentals participated in training. Furthermore, CREC offered limited support services and moved several times during the demonstration, which disrupted program operations. The high total hours for sequential/in-house sites were primarily owing to the Los Angeles Job Corps, which had the highest average hours among all sites. El Centro in Dallas, the other site in this category, ranked only slightly above the average for all sites in total hours.

IV. A Summary of the JOBSTART Experience

The support for the three themes highlighted at the beginning of this chapter can now be summarized using the findings in this chapter. First, the diversity of the sites within the general framework of the JOBSTART guidelines has been a theme of this chapter. A second, related theme, the ability of JOBSTART to be implemented in a variety of settings, has also been discussed. Table

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[30] When dummy variables for type of site were added as independent variables to a regression equation with individual demographic characteristics and a measure of local employment opportunities, the relationships still held.

TABLE 3.11

SUMMARY OF JOBSTART IMPLEMENTATION, BY SITE

Site	Average Total Hours	Average Length of Stay (Months)	Average Hours per Month	Average Hours in		Level of Initial Screening	Rating of		Overall Rating of Implementation	JOBSTART Operating Costs per Experimental ($)[a]
				Education	Training		Job Placement	Support Services		
Concurrent	387	6.1	63	101	276	---	---	---	---	---
Atlanta Job Corps	334	5.8	59	102	177	High	Low	High	Low	4,100[b]
CET/San Jose[c]	335	4.1	82	26[d]	309	Low	High	Medium	High	2,000
Chicago Commons[c]	422	4.3	98	69[d]	353	High	High	Medium	High	6,400
Connelley (Pittsburgh)	538	9.5	57	99	439	Medium	Medium	High	High	5,200
East LA Skills Center	370	5.7	65	76	294	Medium	Low	Medium	Medium	4,900
EGOS (Denver)	270	7.1	38	128	142	Low	Low	Medium	Low	2,000[b]
Phoenix Job Corps	420	6.3	67	161	202	High	High	High	High	4,700[b]
SER/Corpus Christi	400	5.0	80	118	282	Medium	Low	High	Medium	2,100
Sequential/in-house	518	7.0	75	146	276	---	---	---	---	---
El Centro (Dallas)	408	6.0	68	147	179	Medium	Medium	Medium	High	5,100
LA Job Corps	607	7.8	79	146	355	High	Medium	High	High	5,700[b]
Sequential/brokered	307	7.7	39	176	71	---	---	---	---	---
Allentown (Buffalo)	427	10.7	38	238	105	Medium	Low	High	Medium	5,900
BSA (NYC)	282	5.6	49	149	63	Low	Low	Medium	Low	7,500
CREC (Hartford)	171	6.3	29	124	35	Low	Low	Medium	Low	5,200
All sites	398	6.6	60	125	238	---	---	---	---	---

SOURCES: MDRC calculations from JOBSTART enrollment form and MIS data (participation figures); MDRC operations staff (implementation ratings); Appendix C (costs).

NOTES: Calculations for this table used data for all 988 experimentals for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

[a] These cost estimates are rounded to the nearest $100. All costs are in 1986 dollars.

[b] The estimates do not include the cost of providing on-site medical and dental services. The value of these services per JOBSTART experimental was approximately $400 in the Atlanta site, $400 in Phoenix, $600 in Los Angeles, and $24 in Denver.

[c] In this site, education and training were more integrated than in other sites, and staff strongly emphasized training over passing the GED examination.

[d] In this site, some education hours are included in the training component hours.

3.11 illustrates both themes by listing the characteristics of the sites, grouped by program structure (concurrent, sequential/in-house, sequential/brokered). It describes participation in JOBSTART for experimentals by presenting average total hours; average length of stay in the program; average hours per month in the program; and average hours of education and training.[31] Table 3.11 also includes more subjective ratings of implementation, including a rating of the level of initial screening done by each site at intake, of sites' success in implementing job placement and support services (drawn from Table 3.4), and of overall JOBSTART implementation. Finally, the average cost of JOBSTART services per experimental in each site's research sample, listed in the right column of this table, adds to the description of program implementation already presented in this chapter.[32]

The final major theme of the chapter – the diversity of individual experience in JOBSTART – also emerges from the findings presented. Behind the measures of average level of participation there is a wide distribution in the intensity of participation. Subsequent chapters examine whether there are similar differences in impacts.

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[31] Note that each of these measures includes the 11 percent of experimentals who did not participate in the program. All of these items have been included in previous tables except for average hours per month, which is a measure of the extent to which program services were concentrated or spread out over time.

[32] See Appendix C for the details of these cost data, which include items not normally part of program budgets, such as the value of donated goods and services. These cost figures are not net cost figures such as those produced as part of the benefit-cost analysis reported in Chapter 7, which compares the impact of the program to the incremental cost of the services received by experimentals above the level of services received by controls. As discussed in Chapter 4, the controls received substantial non-JOBSTART services; thus, the impact figures estimate the effect of the services received by experimentals above this base of services. Costs used to calculate a benefit-cost ratio would measure the resource cost of the *extra* services received by experimentals. The cost figures reported in Table 3.11 are gross program costs from which the costs of services received by controls are not subtracted.

CHAPTER 4

PARTICIPATION IN EDUCATION AND TRAINING AND PROGRAM IMPACTS ON EDUCATIONAL ATTAINMENT

The goals of the JOBSTART program included increasing participation in education and training activities by a group who otherwise would be little served, and thereby enhancing their educational attainment, employability, and long-term earnings. Chapter 3 described the experimental group's participation in JOBSTART activities.[1] This chapter takes the story a step farther by comparing the participation of these experimentals in JOBSTART (and other programs) to the activities of the control group, to determine whether the offer of JOBSTART services actually led to an increase in participation over what would have occurred otherwise.[2] As will be shown, the youths in the experimental group did participate in more education and training than those in the control group, but the control group was not unserved. Further, participation in JOBSTART did lead to an increase in the rate of receipt of a GED. Chapter 5 examines the extent to which this increase in services and educational attainment led to improvements in employment and earnings by month 48 after random assignment, while Chapter 6 covers impacts on welfare receipt and other outcomes. Chapter 7 uses these findings, in addition to information on program costs, to present a benefit-cost analysis of JOBSTART.[3]

The analysis in Chapters 4 through 7 rests on the random assignment research design (described in earlier chapters) to estimate the differences that JOBSTART made in the lives of those young people given access to it. To evaluate these differences (often called program "impacts"), it is necessary to answer two basic questions. First, on average, what happened to those who were offered access to the program – in this context, the "experimental" group? Second, on average, what would have happened to them had they not been offered access to it, here represented by the experience of the "control" group?[4] The average effect, or impact, of a program is the difference between the two groups in the many outcomes of interest.

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[1] This was based on attendance information reported by the JOBSTART program operators for those in the experimental group.

[2] In addition to using the JOBSTART attendance data from the program MIS, this chapter relies on survey data on non-JOBSTART services for both experimentals and controls gathered 12, 24, and 48 months after random assignment. The JOBSTART MIS data are more accurate about hours attended than are the survey data for other programs, and JOBSTART was a large part of the services received by experimentals. Therefore, the measurement of hours of participation for experimentals is more accurate than for controls.

[3] Chapters 4 through 7 present findings for the full 48-month impact sample as well as for subgroups defined by individual characteristics such as gender.

This chapter addresses several key evaluation questions:

- Did a positive experimental-control difference in receipt of education and training materialize? If so, was this difference maintained over time, or did controls catch up with experimentals by the end of four years after random assignment?
- How did service receipt by experimentals and controls compare among important subgroups such as men, custodial mothers, and all other women in the sample?
- Did JOBSTART produce post-program gains in educational attainment as measured by receipt of a high school diploma or GED? If so, were controls comparable to the experimentals in educational attainment by the fourth year?
- Did the educational attainment of experimentals and controls vary among important subgroups?

To summarize the findings of this chapter, access to JOBSTART did substantially increase the experimentals' participation in education and training activities, raising their rates and average hours of participation well above those of controls, almost half of whom were also active in these types of activities in other programs. This overall difference occurred primarily because of substantially higher participation by experimentals in the first 12 months after random assignment, when many were active in JOBSTART, and was present for all important subgroups in the sample. In the second year of follow-up – months 13 through 24 – experimentals had a slightly higher participation rate than controls. During months 25 through 48, experimentals and controls spent about equal time in education and training activities. Experimentals' greater overall service receipt led to a substantially higher GED receipt rate than among controls, for both the full impact sample and most subgroups.

I. Receipt of Education and Training by Experimentals and Controls

The purpose of the JOBSTART control group was to be a benchmark for measuring program impacts, but if most controls received services similar to those received by experimentals, the benchmark would be useless, and it would be very difficult to evaluate JOBSTART.[5] Because

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[4] Since, as shown in Chapter 2, assignment to JOBSTART was random, there were no systematic differences between experimentals and controls at enrollment; therefore, outcomes for controls could be used to measure what would have happened to experimentals without the program.

[5] The service receipt differences reported here were calculated by comparing the experiences of experimentals and controls, as was done to determine the impact of JOBSTART on such outcomes as (continued...)

JOBSTART targeted disadvantaged dropouts with poor reading skills (a group little served within the JTPA system and elsewhere), demonstration planners anticipated that controls would not be served to any great extent by other programs.

However, JOBSTART controls were expected to receive some services. Even though many performance-driven programs screen out people with low reading levels, JOBSTART control group members were more determined than many school dropouts to pursue services, as evidenced by their volunteering for JOBSTART. In fact, at some point within the 48 months following random assignment, 56.1 percent of controls received non-JOBSTART remedial or occupational instruction. Therefore, the program impacts on educational attainment, employment, earnings, and other outcomes presented in this report are the incremental impacts of JOBSTART over the mix of services available throughout the community to control group members that they pursued on their own initiative. And although controls received substantial services, experimental-control service receipt differences were still large, as is shown in this chapter.

A. In-Program and Post-Program Outcomes

All the events tracked by the JOBSTART MIS and the follow-up surveys (including program participation, GED receipt, employment, and other outcomes) were reckoned from the date of random assignment, not the date of termination from the program.[6] There was a great deal of variation in length of stay in JOBSTART among experimentals, but about 88 percent had stopped participating in the program by the end of month 12 after random assignment. (See Table 3.5.) Thus, in reviewing the findings on program participation and impact estimates, it is useful to think of the first 12 months of follow-up as primarily an in-program period and the months thereafter as primarily post-program.

Table 4.1 shows that, over the four years of follow-up, 94 percent of experimentals and 56.1 percent of controls received some education or training, for an impact of 37.9 percentage points.

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[5](...continued)
educational attainment and employment, analyzed later in this report. These service receipt differences are not normally thought of as program "impacts" because service receipt is the means to reaching the final program goals. However, in this chapter, when discussing experimental-control service receipt differences, the word *impact* has been used to simplify the terminology.

[6] This is a different approach from that used in the Job Corps study (Mallar et al., 1982), in which follow-up began at termination from the program, thereby complicating a comparison of JOBSTART and Job Corps impact findings. But as will become clear in Chapter 5, starting follow-up at program entry allows for a careful analysis of the foregone earnings resulting from participation in the program.

TABLE 4.1

IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 48

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	p[a]
Ever received any education or training, months 1-48	94.0	56.1	37.9***	0.000
Ever received any education or training, months 1-12	90.4	26.2	64.2***	0.000
Ever received any education or training, months 13-24	31.8	25.8	6.0***	0.003
Ever received any education or training, months 25-48	30.8	31.4	-0.6	0.777
Sample size	988	953		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

This impact was statistically significant, that is, too big to be likely to have arisen entirely by chance. Its significance is indicated by the stars next to the impact estimate (the "difference") and the p-value, which is virtually equal to zero, signifying that the likelihood that the observed difference occurred because of random error is very low. During the first follow-up year, the difference was large and significant (64.2 percentage points); during the second it declined as JOBSTART participation ended (to 6 percentage points); and in the latter two years an almost equal number of experimentals and controls received education or training. On a monthly basis (not shown in the table), the proportion of experimentals in programs, mainly JOBSTART, was highest during the first three months after random assignment and fell rather steadily over time, to 11.2 percent during month 24. The proportion of controls in programs was much smaller at the beginning of the follow-up period, peaked at about 15 percent during months 10 through 13, and soon thereafter reached a plateau at about 10 to 12 percent, about even with the level to which experimentals had fallen.

Hours of education or training followed a similar pattern. Table 4.2 shows that over the four-year period experimentals participated an average of 800 hours and controls an average of 432 hours, for an impact of 367 hours.[7] The difference was largest in months 1 through 12 (300 hours), when most experimentals were active in JOBSTART; dropped to 65 hours in months 13 through 24; and was virtually zero over the final two years of follow-up. On a monthly basis (not shown in the table), average experimental hours peaked at about 70 hours per month during month two and then fell steadily, while control hours, always much lower, peaked during month nine and then stabilized at about 10 hours per month for the remainder of the first two years of follow-up before dropping to very low levels in the final two years.

Among the 94 percent of experimentals and 56.1 percent of controls who received any services during the four-year period – that is, excluding those who received no services – experimentals averaged 882 hours and controls 800 hours, a difference of 82 hours.[8] Because so many fewer controls received services, and those who did were probably quite motivated, this difference in hours *for those who received services* may understate the intensity of participation among served experimentals. The served experimentals who were directly comparable in motivation to the served controls might well have been those who received many more than the average hours for all served experimentals.

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[7] It is important to remember that the average total hours for experimentals and controls shown in Table 4.2 *include data for sample members who did not participate and therefore had zero hours.* Other information included below presents average total hours only for those active in some program.

[8] As mentioned above, this finding is not shown in Table 4.2.

TABLE 4.2

IMPACTS ON TOTAL HOURS OF EDUCATION OR TRAINING RECEIVED THROUGH MONTH 48

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Total hours of education or training received, months 1-48	799.59	432.44	367.15***	0.000
Total hours of education or training received, months 1-12	415.66	115.47	300.19***	0.000
Total hours of education or training received, months 13-24	155.37	90.24	65.13***	0.000
Total hours of education or training received, months 25-48	228.57	226.73	1.84	0.955
Sample size	988	953		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

For experimentals, "hours of education or training" include JOBSTART hours from MIS data and non-JOBSTART hours from survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

In summary, a service receipt differential between experimentals and controls did materialize. Not only did experimentals receive education and training at vastly higher rates than controls throughout the first half of the 48-month follow-up period, but they also on average received many more hours. However, as noted earlier, the control group did receive some similar services from other programs in the community. Thus, the findings presented in Chapters 4, 5, and 6 represent the incremental impacts of JOBSTART above the existing level of service.

B. Differences in Receipt of Education and Training by Men and Women

Table 4.3 splits the 48-month sample into three groups: men, women living with children of their own at random assignment (custodial mothers), and all other women in the sample; the impacts on service receipt are presented for each group using the same methods as in Tables 4.1 and 4.2. In general, approximately equal percentages of experimentals in all three groups were active in all types of education and training activities in the first year, when most activity occurred.[9] However, there were clear differences in participation among the control groups for the three subgroups: Men in the control group participated in education and training less than custodial mothers, who in turn participated less than all other women.

The larger differences in participation rates and average hours between experimentals and controls for men, therefore, result largely from the lower level of activity of the control group. During the four-year follow-up period, 94.3 percent of men in the experimental group and 51.3 percent in the control group were active in education or training, for a difference of 43 percentage points. This resulted in a difference of 392 average total hours between men in the experimental and control groups. For women living with their own children, more experimentals were active than controls (95 percent versus 59.7 percent) during the four-year follow-up period, and experimentals averaged 331 more hours than controls. For other women, the comparable figures are 93.2 percent versus 60.9 percent, and 337 more hours for experimentals. For all three of these groups, in the third and fourth years of follow-up, participation in education or training by both experimentals and controls declined sharply and the difference in their participation was no longer statistically significant.

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[9] This finding is based on an analysis of the proportion of experimentals in each group who ever participated in an activity during the period in question. The finding holds during the first year of follow-up and over the entire 48-month follow-up period as well. In the second year of follow-up (and over the entire two-year follow-up period), more "other women" (women who were not custodial mothers) participated in an activity (registering the highest average total hours in the second year). Custodial mothers had the next highest average total hours and men had the lowest. These differences were largely attributable to differences in participation in non-JOBSTART activities. As reported in Chapter 3, participation in JOBSTART activities was similar among the three groups.

TABLE 4.3

IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 48, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Ever received any education or training (%)				
Months 1-48	94.3	51.3	43.0***	0.000
Months 1-12	89.7	21.9	67.7***	0.000
Months 13-24	27.4	18.6	8.9***	0.002
Months 25-48	26.1	30.1	-4.1	0.180
Total hours of education or training received				
Months 1-48	758.25	366.56	391.69***	0.000
Months 1-12	423.64	84.05	339.59***	0.000
Months 13-24	141.79	60.51	81.28***	0.000
Months 25-48	192.83	222.0	-29.17	0.547
Sample size	448	452		
Women living with own child(ren)				
Ever received any education or training (%)				
Months 1-48	95.0	59.7	35.3***	0.000
Months 1-12	91.4	26.0	65.4***	0.000
Months 13-24	35.2	30.5	4.7	0.271
Months 25-48	39.1	34.1	5.0	0.254
Total hours of education or training received				
Months 1-48	776.68	445.22	331.46***	0.000
Months 1-12	388.76	113.33	275.43***	0.000
Months 13-24	161.45	94.40	67.04**	0.028
Months 25-48	226.47	237.49	-11.02	0.856
Sample size	257	251		

(continued)

TABLE 4.3 (continued)

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Women not living with own child(ren), including those who did not have any				
Ever received any education or training (%)				
Months 1-48	93.2	60.9	32.3***	0.000
Months 1-12	91.0	33.8	57.2***	0.000
Months 13-24	37.1	32.4	4.7	0.247
Months 25-48	31.2	30.7	0.5	0.898
Total hours of education or training received				
Months 1-48	881.29	543.90	337.39***	0.000
Months 1-12	426.45	175.60	250.86***	0.000
Months 13-24	179.45	130.64	48.81	0.135
Months 25-48	275.39	237.66	37.73	0.578
Sample size	283	250		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities. For experimentals, "hours of education or training" include JOBSTART hours from MIS data and non-JOBSTART hours from survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

C. Differences in Receipt of Education and Training by Other Subgroups

The primary goal of the JOBSTART evaluation is to estimate the difference that access to the program made for its target population (in other words, the program's impacts). While the size of the full 48-month sample is large enough to estimate overall impacts on policy-relevant outcomes, it provides considerably less statistical power for estimating subgroup impacts and differences in impacts among subgroups. Keeping this limitation in mind, this section presents an analysis of the difference in service receipt by experimentals and controls for various subgroups and compares the size of this difference among the selected subgroups.

To summarize the findings in this section: The service receipt differences between experimentals and controls observed for the full sample were present and large for virtually all important subgroups. The observed differences in service receipt impacts among subgroups primarily reflect variation in the level of service receipt of controls.

The impacts presented in Table 4.3 represent split-sample analysis, discussed in Chapter 2, which does not control for differences in baseline characteristics other than the characteristic used to define the subgroup. Table 4.4 uses the second method of analysis described in Chapter 2 to calculate within-subgroup impacts and between-subgroup impact differences for the most important measure of program activity: receipt of education or training during the four-year follow-up period. This method controls for differences in baseline characteristics other than that used to define the subgroup.[10]

For example, the first three rows of Table 4.4 present impacts for women and men using statistical techniques to control for gender differences in factors such as employment experience, educational level, ethnicity, and parental status. It thus shows a comparison of the impacts by gender with other characteristics held constant. The first row of Table 4.4, in the column labeled "subgroup impact difference," shows that the impact for women was 7.6 percentage points below the impact for men, and this difference in service receipt impacts was statistically significant. The "subgroup impact" column shows that this difference in impacts was calculated as 36.2 percentage

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[10] The impact estimates in Table 4.4 were calculated by conducting a two-way analysis of covariance, controlling for differences in pre-random assignment characteristics other than the characteristic used to define the subgroup (see Ostle, 1975, p. 461). The adjustments were done using a linear regression model. Characteristics that affect outcomes and impacts with a nonlinear relationship are not controlled for with this procedure, and there is no procedure that can control for unmeasured characteristics that affect outcomes and impacts. The adjusted outcomes for men, custodial mothers, and other women presented in Table 4.4 are very similar to the unadjusted outcomes in Table 4.3, but the adjustments do make more of a difference for many of the other subgroups listed in the table.

TABLE 4.4

IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 48, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

		Ever Received Any Education or Training, Months 1-48					
Characteristic and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Gender						-7.6**	0.044
Women	1,041	89.1	60.5	28.6***	0.000	---	---
Men	900	87.3	51.1	36.2***	0.000	---	---
Ethnicity						---	0.633
White, non-Hispanic	172	88.8	51.0	37.8***	0.000	---	---
Black, non-Hispanic	860	88.3	58.2	30.1***	0.000	---	---
Hispanic	847	87.6	55.1	32.5***	0.000	---	---
Other	62	93.7	54.9	38.8***	0.000	---	---
Ethnicity, by gender						---	0.489
Women							
White, non-Hispanic	97	85.1	50.7	34.4***	0.000	---	---
Black, non-Hispanic	467	89.6	62.6	27.0***	0.000	---	---
Hispanic	451	89.3	60.5	28.8***	0.000	---	---
Other	26	94.8	61.4	33.4**	0.044	---	---
Men							
White, non-Hispanic	75	96.6	50.9	45.7***	0.000	---	---
Black, non-Hispanic	393	87.3	53.4	33.9***	0.000	---	---
Hispanic	396	85.6	48.8	36.8***	0.000	---	---
Other	36	92.1	49.6	42.5***	0.003	---	---
Parental status						---	0.238
Women living with own child(ren)							
No	533	89.9	62.0	27.9***	0.000	---	---
Yes	508	87.3	58.0	29.3***	0.000	---	---
Men who have own child(ren)							
No	785	88.2	52.3	35.9***	0.000	---	---
Yes	115	85.5	47.0	38.5***	0.000	---	---

(continued)

TABLE 4.4 (continued)

		Ever Received Any Education or Training, Months 1-48					
Characteristic and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Employed within past year						2.2	0.560
No	914	88.3	55.0	33.3***	0.000	---	---
Yes	1,027	88.2	57.1	31.1***	0.000	---	---
Prior employment, by gender						---	0.075
Women employed within past year							
No	583	92.0	59.7	32.3***	0.000	---	---
Yes	458	85.5	61.5	24.0***	0.000	---	---
Men employed within past year							
No	331	83.2	48.1	35.1***	0.000	---	---
Yes	569	89.9	52.7	37.2***	0.000	---	---
Left school in grade 11 or 12						-0.8	0.850
No	1,140	88.1	56.3	31.8***	0.000	---	---
Yes	801	88.3	55.7	32.6***	0.000	---	---
Received occupational training within past year						3.7	0.462
No	1,615	88.7	56.0	32.7***	0.000	---	---
Yes	326	85.7	56.7	29.0***	0.000	---	---
Age						2.6	0.538
16-19	1,425	88.8	56.0	32.8***	0.000	---	---
20 or 21	516	86.6	56.4	30.2***	0.000	---	---
Age, by gender						---	0.216
Women							
16-19	763	89.7	60.6	29.1***	0.000	---	---
20 or 21	278	87.0	60.0	27.0***	0.000	---	---
Men							
16-19	662	87.9	50.8	37.1***	0.000	---	---
20 or 21	238	86.0	52.2	33.8***	0.000	---	---

(continued)

TABLE 4.4 (continued)

Characteristic and Subgroup	Sample Size	Ever Received Any Education or Training, Months 1-48: Experimentals (%)	Ever Received Any Education or Training, Months 1-48: Controls (%)	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Marital status						1.7	0.793
Ever married	184	89.8	56.1	33.7***	0.000	---	---
Never married	1,757	88.1	56.1	32.0***	0.000	---	---
Living in own household or with boy/girlfriend						4.9	0.312
No	1,575	89.0	56.0	33.0***	0.000	---	---
Yes	366	84.7	56.6	28.1***	0.000	---	---
Own AFDC case or receiving General Assistance						7.6*	0.074
No	1,418	87.2	53.0	34.2***	0.000	---	---
Yes	523	90.8	64.2	26.6***	0.000	---	---
Own AFDC case						6.9	0.132
No	1,522	87.7	54.1	33.6***	0.000	---	---
Yes	419	89.8	63.1	26.7***	0.000	---	---
Receiving Food Stamps						5.5	0.166
No	1,214	90.2	56.0	34.2***	0.000	---	---
Yes	727	85.0	56.3	28.7***	0.000	---	---
Arrested since age 16						1.6	0.761
No	1,649	88.2	55.9	32.3***	0.000	---	---
Yes	292	88.0	57.3	30.7***	0.000	---	---
Arrested since age 16, by gender						---	0.167
Women							
No	986	89.1	60.4	28.7***	0.000	---	---
Yes	55	88.3	61.1	22.2**	0.017	---	---
Men							
No	663	88.0	50.2	37.8***	0.000	---	---
Yes	237	85.6	54.0	31.6***	0.000	---	---

(continued)

TABLE 4.4 (continued)

Characteristic and Subgroup	Sample Size	Ever Received Any Education or Training, Months 1-48		Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
		Experimentals (%)	Controls (%)				
Lived with both parents at age 14						-4.7	0.232
No	1,264	86.6	56.1	30.5***	0.000	---	---
Yes	677	91.4	56.2	35.2***	0.000	---	---
Reason for leaving regular high school						---	0.286
School-related	925	86.9	51.6	35.3***	0.000	---	---
Job-related	197	85.3	58.1	27.2***	0.000	---	---
Other	819	90.5	60.4	30.1***	0.000	---	---

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.

points (the impact for men, shown in the third row) minus 28.6 percentage points (the impact for women, shown in the second row). For characteristics with more than two subgroups, such as ethnicity, the subgroup impact column shows the experimental-control service receipt difference for each subgroup (for example, 37.8 percentage points for whites), but no subgroup impact difference is presented; when there are more than two subgroups, no single impact difference among them can be calculated.

In addition to gender, important characteristics for subgroup comparisons of impacts on service receipt include:

- **Age.** In the general youth population, different patterns of labor market behavior are exhibited at each age. Labor force participation, employment, and earnings often increase from age 16 to the early twenties. Thus, holding everything else constant, youths over age 20 are likely to pay higher opportunity costs for program attendance than younger participants. These likely differences in labor market experiences did not affect the impacts on service receipt, with 16- to 19-year-old sample members having virtually the same impact on service receipt (32.8 percentage points) as 20- to 21-year-old youths (30.2 percentage points).

- **Prior employment.** For those with a more extensive work history, as evidenced by employment in the year before random assignment, the opportunity costs of participating in an employment and training program may be greater. Experimentals with and without prior-year employment had high rates of participation in education and training and similar impacts on service receipt.[11]

- **Highest grade attended.** While all JOBSTART enrollees were high school dropouts, some left school before completing the tenth grade, while others dropped out during their junior or senior year. Despite the differences in past success in school, the levels of participation and impacts on service receipt were nearly identical for the two subgroups. Apparently JOBSTART sites found ways to engage the youths with lower educational attainment in the program.

- **Welfare receipt.**[12] Those who receive AFDC, General Assistance, or Home Relief may tend to get higher levels of support services such as child care, and sometimes may be mandated to participate in some type of program to maintain eligibility for their cash benefits. Impacts for those not receiving welfare at random assignment were higher than for welfare recipients because control group welfare recipients were more likely to participate in an education or training program.

- **Reason for leaving school.** Among the subgroups in this category, those youths who dropped out of school for reasons related to their educational experience (as opposed to a desire to get a job or the birth of a child) had the largest service receipt impact.

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[11] For men, the difference in service receipt was somewhat greater for those without prior-year employment, while for women, those with and without recent employment had similar service receipt impacts.

[12] Since only about 13 percent of the men in the sample received AFDC or General Assistance at random assignment, the subgroup receiving welfare was primarily made up of women.

D. Receipt of Education and Training Separately

Surveys at 12 and 24 months after random assignment collected information on monthly participation in education and training separately. These data (not shown in a table) showed a large difference between experimentals and controls in participation in education throughout the first half of the four-year follow-up period. During the first year, 88 percent of experimentals participated in education compared to 15 percent of controls, for a difference of 73 percentage points. In the second year, the participation of experimentals dropped sharply as they left the JOBSTART program, while that of controls remained approximately what it was during the first year, causing the service receipt difference to decline to only 6 percentage points. Analysis of subgroup impacts again indicates a somewhat stronger service receipt differential for men than for women, because female controls tended to get more education services on their own than did male controls.

JOBSTART had a smaller impact on the receipt of training; it achieved less of an experimental-control differential for training alone than for training and education together.[13] The smaller impact for training than for education resulted from the failure of some JOBSTART sites, notably the sequential/brokered sites, to move participants from the education to the training component (see Chapter 3). Similar patterns of impacts on receipt of training appeared for men and women, but again the impacts tended to be larger for men.[14]

II. Impacts on Educational Attainment

As indicated in the 1989 interim report, *Implementing JOBSTART*, the impacts of JOBSTART on educational attainment during the in-program period (that is, the first 12 months of follow-up) were quite similar to those of the program that inspired it, the residential Job Corps. An evaluation of the Job Corps found that 24 percent of Corpsmembers, but only 5 percent of the comparison group, had high school diplomas or GEDs six months after termination from the program (the period roughly equivalent to a year of post-random assignment follow-up).[15] At a similar point in the JOBSTART follow-up, 27.5 percent of the experimental group and 9.9 percent of the control

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[13] The two-year impact for the full sample on receipt of training alone was 41.6 percentage points, as compared to 53 percentage points for education or training.

[14] The impact on training receipt was 36 percentage points for women and 49 percentage points for men.

[15] Mallar et al., 1982. See also Betsey et al., 1985, p. 112.

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group had a GED or high school diploma, for an impact of 17.6 percentage points. This report carries the JOBSTART story forward to the end of the four-year follow-up period.

JOBSTART impacts on educational attainment for the full 48-month sample are presented in Table 4.5. Table 4.6 presents separate split-sample results for men, women living with their own children at random assignment, and all other women. The severe and intractable problems in reading and mathematics for the young adults in JOBSTART are reflected in the low rates of completing high school or passing the GED examination for both experimentals and controls. By month 48, 28.6 percent of all controls had received a high school diploma or passed the GED examination (see Table 4.5), with other women having the highest percentage among the three subgroups in Table 4.6. Among all experimentals, 42 percent had attained a high school diploma or GED, with about equal proportions among the three subgroups.

The full-sample impact on attainment of a GED or high school diploma by month 48 was 13.4 percentage points. Most of the increased educational attainment for both the experimental and control groups came in the first 24 months of follow-up. At that point, the rate of GED or high school diploma receipt was 36 percent for experimentals and 21 percent for controls, for an impact of 15 percentage points (not shown in the table).

Among the three key subgroups shown in Table 4.6, the impact over the 48-month follow-up period on attainment of a GED or high school diploma was 13.7 percentage points for men, 15.3 percentage points for custodial mothers, and 10.4 percentage points for all other women.

Compared to the impacts on attainment of either a high school diploma or GED, the impacts on attainment of GEDs only were slightly larger. This was the case because controls (who, as a rule, were not participating in an alternative education program similar to JOBSTART) were slightly more likely to return to regular high school than experimentals, although it was rare for either controls or experimentals. The 48-month impact on GED receipt was 16.5 percentage points for the full sample, 15.9 percentage points for men, 15.7 percentage points for custodial mothers, and 16.8 percentage points for other women.[16]

These large educational attainment impacts were present for many different subgroups in the

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[16] The JOBSTART surveys also asked about receipt of a trade certificate or license during the follow-up period. Because there were not consistent standards, the precise significance of these certificates and licenses is not clear. Some could have been awarded for completion of a program, rather than for achievement of a generally recognized occupational competency. Nevertheless, 33.1 percent of experimentals received such certificates and licenses, compared to 17.3 percent of controls, for an impact of 15.8 percentage points. The impact was slightly larger than this for men, and smallest for custodial mothers.

TABLE 4.5

IMPACTS ON EDUCATIONAL ATTAINMENT
THROUGH MONTH 48

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	p[a]
Received GED by end of month 48	37.6	21.1	16.5***	0.000
Received GED or high school diploma by end of month 48	42.0	28.6	13.4***	0.000
Received trade certificate or license by end of month 48	33.1	17.3	15.8***	0.000
Received associate's or 2-year college degree by end of month 48	0.5	0.3	0.2	0.484
Received bachelor's or 4-year college degree by end of month 48	0.0	0.1	-0.1	0.291
Sample size	988	953		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 4.6

IMPACTS ON EDUCATIONAL ATTAINMENT THROUGH MONTH 48, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Received GED by end of month 48 (%)	36.8	20.9	15.9***	0.000
Received GED or high school diploma by end of month 48 (%)	42.0	28.3	13.7***	0.000
Received trade certificate or license by end of month 48 (%)	35.9	14.9	21.0***	0.000
Received associate's or 2-year college degree by end of month 48 (%)	0.2	0.2	0.0	0.926
Received bachelor's or 4-year college degree by end of month 48 (%)	0.0	0.2	-0.2	0.375
Sample size	448	452		
Women living with own child(ren)				
Received GED by end of month 48 (%)	39.1	23.4	15.7***	0.000
Received GED or high school diploma by end of month 48 (%)	42.0	26.7	15.3***	0.000
Received trade certificate or license by end of month 48 (%)	32.1	21.3	10.8***	0.006
Received associate's or 2-year college degree by end of month 48 (%)	1.3	0.2	1.1	0.177
Received bachelor's or 4-year college degree by end of month 48 (%)	0.0	0.0	0.0	1.000
Sample size	257	251		

(continued)

TABLE 4.6 (continued)

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Women not living with own child(ren), including those who did not have any				
Received GED by end of month 48 (%)	36.9	20.2	16.8***	0.000
Received GED or high school diploma by end of month 48 (%)	41.6	31.3	10.4**	0.012
Received trade certificate or license by end of month 48 (%)	30.0	17.2	12.8***	0.001
Received associate's or 2-year college degree by end of month 48 (%)	0.4	0.4	0.0	0.924
Received bachelor's or 4-year college degree by end of month 48 (%)	0.0	0.0	0.0	1.000
Sample size	283	250		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 4.7

IMPACTS ON GED ATTAINMENT THROUGH MONTH 48, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

		Received GED by End of Month 48					
Characteristic and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Gender						0.5	0.900
Women	1,041	38.4	21.7	16.7***	0.000	---	---
Men	900	36.8	20.6	16.2***	0.000	---	---
Ethnicity						---	0.842
White, non-Hispanic	172	51.6	32.1	19.5***	0.003	---	---
Black, non-Hispanic	860	38.1	21.0	17.1***	0.000	---	---
Hispanic	847	35.0	19.2	15.8***	0.000	---	---
Other	62	27.5	19.3	8.2	0.462	---	---
Ethnicity, by gender						---	0.996
Women							
White, non-Hispanic	97	56.7	38.2	18.5**	0.037	---	---
Black, non-Hispanic	467	39.4	20.9	18.5***	0.000	---	---
Hispanic	451	35.0	20.0	15.0***	0.000	---	---
Other	26	15.0	9.5	5.5	0.749	---	---
Men							
White, non-Hispanic	75	43.1	25.8	17.3*	0.092	---	---
Black, non-Hispanic	393	36.3	20.9	15.4***	0.000	---	---
Hispanic	396	35.2	18.5	16.7***	0.000	---	---
Other	36	39.0	24.7	14.3	0.336	---	---
Parental status						---	0.928
Women living with own child(ren)							
No	533	38.3	21.1	17.2***	0.000	---	---
Yes	508	39.7	23.5	16.2***	0.000	---	---
Men who have own child(ren)							
No	785	36.1	20.6	15.5***	0.000	---	---
Yes	115	35.3	14.2	21.1***	0.010	---	---

(continued)

TABLE 4.7 (continued)

		Received GED by End of Month 48					
Characteristic and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Employed within past year						-2.0	0.598
No	914	35.0	19.6	15.4***	0.000	---	---
Yes	1,027	39.9	22.5	17.4***	0.000	---	---
Prior employment, by gender						---	0.952
Women employed within past year							
No	583	34.6	18.7	15.9***	0.000	---	---
Yes	458	42.2	24.6	17.6***	0.000	---	---
Men employed within past year							
No	331	36.0	21.7	14.3***	0.003	---	---
Yes	569	37.8	20.8	17.0***	0.000	---	---
Left school in grade 11 or 12						2.6	0.521
No	1,140	36.7	19.2	17.5***	0.000	---	---
Yes	801	38.9	24.0	14.9***	0.000	---	---
Received occupational training within past year						0.4	0.945
No	1,615	37.8	21.3	16.5***	0.000	---	---
Yes	326	36.6	20.5	16.1***	0.001	---	---
Age						-2.1	0.643
16-19	1,425	38.2	22.3	15.9***	0.000	---	---
20 or 21	516	36.1	18.1	18.0***	0.000	---	---
Age, by gender						---	0.967
Women							
16-19	763	39.0	23.1	15.9***	0.000	---	---
20 or 21	278	36.2	17.4	18.8***	0.000	---	---
Men							
16-19	662	37.2	21.3	15.9***	0.000	---	---
20 or 21	238	35.9	19.0	16.9***	0.003	---	---

(continued)

TABLE 4.7 (continued)

		Received GED by End of Month 48					
Characteristic and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Marital status						12.8*	0.057
Ever married	184	47.9	19.8	28.1***	0.000	---	---
Never married	1,757	36.6	21.3	15.3***	0.000	---	---
Living in own household or with boy/girlfriend						1.4	0.781
No	1,575	37.4	20.7	16.7***	0.000	---	---
Yes	366	38.2	22.9	15.3***	0.001	---	---
Own AFDC case or receiving General Assistance						2.8	0.529
No	1,418	39.1	21.9	17.2***	0.000	---	---
Yes	523	33.3	18.9	14.4***	0.000	---	---
Own AFDC case						1.1	0.817
No	1,522	38.8	22.1	16.7***	0.000	---	---
Yes	419	33.1	17.5	15.6***	0.000	---	---
Receiving Food Stamps						0.1	0.971
No	1,214	37.0	20.5	16.5***	0.000	---	---
Yes	727	38.7	22.3	16.4***	0.000	---	---
Arrested since age 16						-4.2	0.459
No	1,649	37.8	22.0	15.8***	0.000	---	---
Yes	292	36.2	16.2	20.0***	0.000	---	---
Arrested since age 16, by gender						---	0.781
Women							
No	986	38.4	22.2	16.2***	0.000	---	---
Yes	55	45.2	17.9	27.3**	0.022	---	---
Men							
No	663	37.3	21.9	15.4***	0.000	---	---
Yes	237	33.7	15.0	18.7***	0.001	---	---

(continued)

TABLE 4.7 (continued)

Characteristic and Subgroup	Sample Size	Received GED by End of Month 48: Experimentals (%)	Received GED by End of Month 48: Controls (%)	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Lived with both parents at age 14						-4.4	0.281
No	1,264	37.5	22.6	14.9***	0.000	---	---
Yes	677	37.9	18.6	19.3***	0.000	---	---
Reason for leaving regular high school						---	0.958
School-related	925	36.8	20.1	16.7***	0.000	---	---
Job-related	197	43.6	25.6	18.0***	0.004	---	---
Other	819	37.2	21.1	16.1***	0.000	---	---

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.

sample, as evident in Table 4.7, which shows 48-month impacts on GED receipt.[17] For example, the first three rows of the table present impacts on GED receipt for women and men. For women, the impact was 16.7 percentage points, while for men it was 16.2 percentage points. The difference in impacts for these two groups, 0.5 percentage points, is reported in the column labeled "subgroup impact difference," and is not statistically significant. Other subgroup findings include impacts of more than 15 percentage points for whites, blacks, and Hispanics; an impact of 17.5 percentage points for youths who left school in ninth or tenth grade, and 14.9 percentage points for those who left in eleventh or twelfth grade; similar impacts for those who left school for job-related (18 percentage points) or school-related (16.7 percentage points) reasons, and for those with and without an arrest since age 16 (20 and 15.8 percentage points, respectively); and a 15.6 percentage point impact for youths with their own AFDC case at random assignment.

JOBSTART did increase the educational attainment of young school dropouts, just as it increased their receipt of education and training services. However, many members of the experimental group remained without a GED or high school diploma at the end of the follow-up period. Together, these two major findings lead to an important question: Did JOBSTART – by providing additional education and training and increasing educational attainment – make a difference in the employment, earnings, and other key outcomes of youths? Chapters 5 and 6 address this issue for the full sample and subgroups.

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[17] The findings presented in this paragraph are based on an analysis similar to that used for Table 4.4: that is, impacts are for subgroups defined by a specific characteristic, with differences in the other observed characteristics statistically controlled for through linear regression.

CHAPTER 5

IMPACTS ON EARNINGS AND EMPLOYMENT

Chapter 4 showed that JOBSTART had strong impacts on the receipt of a high school diploma or GED certificate for most subgroups of the 48-month impact sample, that these impacts persisted over time, and that the vast majority of those who earned a high school diploma or GED had already done so by the end of the second year after random assignment. This chapter focuses on the extent to which the impact on educational attainment translated, in turn, into greater success in the labor market by the end of the fourth follow-up year. The findings on earnings and employment presented here are not the full story for the JOBSTART sample because the individuals are still in only their early to mid-twenties. The findings are, however, the final results for the JOBSTART project because there are no plans to gather additional follow-up data.

The following key questions about labor market outcomes are addressed for the full 48-month impact sample and for important subgroups:

- What sacrifices of earnings or employment opportunities did experimentals make during the first year after random assignment, which for many was primarily a period spent in JOBSTART?
- Had the experimentals begun to catch up with the controls in earnings and employment by the end of the second year? The third year? The fourth year?
- Did the cumulative earnings impact turn positive at some point during the four years? If so, when? Did it continue to grow thereafter?
- Do more precise measures of work effort (employment rates, hours worked, and wages earned per hour) shed additional light on the basic earnings impacts during the four-year follow-up period?
- How did experimentals and controls compare in earnings and employment within important subgroups? Were the labor market effects of JOBSTART different for men, women who were custodial mothers, and other women? Did JOBSTART's impacts on earnings vary according to age, grade at the time of dropout, reason for dropping out of school, criminal record, or other characteristics of the young people in the sample?

The next chapter presents a similar analysis for other outcomes, such as public assistance receipt, childbearing, criminal arrests, and drug use, again with subanalyses for key subgroups. It concludes with a summary of all the measured impacts for the key subgroups. In both this chapter and the next it is important to recognize that dividing the sample into smaller subgroups reduces the likelihood that the impact estimates will be statistically significant. We present the subgroup findings – despite their reduced statistical power – because of the strong interest in learning how the program worked for different groups within the JOBSTART population.

I. Impacts on Earnings

The overall impact of JOBSTART on the earnings of the 48-month impact sample was a statistically insignificant gain of $214, or 1.3 percent of what the experimentals would have earned over the four years after random assignment if they had not had access to JOBSTART. This overall impact is a weighted average of a small loss for men and gains of several hundred dollars each for both custodial mothers and all other women. None of these subgroup impacts was statistically significant. The time path of the estimated impacts indicates that all three groups made some sacrifices in earnings during the first, predominantly in-program, year; further sacrifices for men, but not for either group of women, during the second year; and small gains for all three groups during years three and four.

Table 5.1 summarizes JOBSTART's impacts on earnings for the full 48-month impact sample. As shown in the first row, JOBSTART raised, by an average of $214, cumulative earnings among the 1,941 sample members (of the 2,311 at baseline) who had four years of continuous follow-up data. Specifically, control group members earned an average of $16,796 over the four-year span, while experimentals earned an average of $17,010, or 1.3 percent more than the control group mean.[1] The $214 impact was not statistically significant, however. The $214 estimated gain arose from positive impacts during the third and fourth years following random assignment of $423 and $410, respectively, which were partially offset by impacts of -$499 during the first year and -$121 during the second year of follow-up. Only the first-year estimate was statistically significant.

The three panels of Table 5.2 show the results of separate impact calculations for men, custodial mothers, and all other women. Although none of the estimated impacts on cumulative four-year earnings for the three subgroups was statistically significant, point estimates were positive for both

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[1] Note that the averages for the control group and the experimental group include zero earnings for those who did not work. The average for the experimentals includes data on those who had access to JOBSTART but did not participate, who made up 11 percent of all experimentals. Based on the assumption that nonparticipants were unaffected by JOBSTART or other programs, impacts on participants would be 12 percent higher than the total reported in the text for all experimentals. See Appendix B for the details of this adjustment.

TABLE 5.1

IMPACTS ON ANNUAL EARNINGS THROUGH MONTH 48

Outcome and Follow-Up Period	Experimentals ($)	Controls ($)	Difference ($)	p[a]
Total earnings				
Years 1-4	17,010	16,796	214	0.757
Earnings				
Year 1	2,097	2,596	-499***	0.001
Year 2	3,991	4,112	-121	0.563
Year 3	5,329	4,906	423	0.102
Year 4	5,592	5,182	410	0.125
Sample size	988	953		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 5.2

IMPACTS ON ANNUAL EARNINGS THROUGH MONTH 48, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Total earnings ($)				
Years 1-4	23,364	23,637	-273	0.818
Year 1	2,929	3,741	-812***	0.003
Year 2	5,435	5,831	-396	0.266
Year 3	7,401	6,957	444	0.320
Year 4	7,599	7,107	492	0.271
Sample size	448	452		
Women living with own child(ren)				
Total earnings ($)				
Years 1-4	8,959	8,334	625	0.557
Year 1	1,016	1,160	-144	0.451
Year 2	2,097	1,947	150	0.648
Year 3	2,700	2,372	328	0.409
Year 4	3,146	2,856	290	0.505
Sample size	257	251		
Women not living with own child(ren), including those who did not have any				
Total earnings ($)				
Years 1-4	13,923	13,310	613	0.609
Year 1	1,697	2,040	-343	0.126
Year 2	3,345	3,269	76	0.831
Year 3	4,309	3,889	420	0.360
Year 4	4,572	4,111	461	0.342
Sample size	283	250		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

groups of women but negative for men.[2] Custodial mothers in the experimental group gained $625, or 7.5 percent of their control group's mean of $8,334. Other women in the experimental group gained $613, or 4.6 percent of their control group's mean of $13,310. And the men lost $273, or -1.2 percent of their control group's mean of $23,637.

For all three groups, the impact estimates were negative during the first, predominantly in-program, year. The custodial mothers sacrificed $144 during that first year, other women sacrificed $343, and men gave up $812 in earnings. These differences in initial impacts in part reflect the sharp differences in earnings across the control groups; men in the control group earned much more than women who were not custodial mothers, who in turn earned considerably more than custodial mothers. Although the experimental-control difference in earnings remained negative for men during the second year, it turned slightly positive for both groups of women during that time. By the third year, experimental-control differences were positive for all three groups, and they remained positive during the fourth year. These statistically insignificant differences fell slightly in magnitude from year three to year four for custodial mothers, but grew slightly for men and other women. During the final year of follow-up, the experimental-control differences in earnings were $290 (or 10.2 percent) for custodial mothers, $461 (11.2 percent) for other women, and $492 (6.9 percent) for men.

Table 5.3 presents earnings impact estimates for male sample members arrested between age 16 and random assignment and for all youths in the sample according to their reason for dropping out of regular high school. Impacts for males arrested since age 16 (shown in the top panel of the table) were positive in years two through four of follow-up and for the full four years, with statistically significant impacts in the fourth year despite the small sample size. Although the small sample size of this group means that caution must be exercised in interpreting the subgroup impact findings, it does appear that JOBSTART had noticeably better impacts for young males with a prior arrest than other young males, for whom overall earnings impacts remained negative at the four-year point.

JOBSTART's strong positive impacts for young males arrested since age 16 are in stark contrast to findings from the National JTPA Study at the 18-month point. In that study, the impacts of JTPA-funded Title IIA services were noticeably worse for young out-of-school males with

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[2] The impacts for all women (combining the two female subgroups) were also statistically insignificant.

TABLE 5.3

IMPACTS ON ANNUAL EARNINGS THROUGH MONTH 48, FOR KEY SUBGROUPS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men arrested since age 16				
Total earnings ($)				
Years 1-4	22,835	20,344	2,491	0.313
Year 1	3,091	4,027	-936	0.110
Year 2	5,722	5,297	425	0.605
Year 3	7,052	5,923	1,129	0.187
Year 4	6,970	5,098	1,872**	0.039
Sample size	127	110		
Full sample, by reason for leaving regular high school				
School-related				
Total earnings ($)				
Years 1-4	17,590	16,409	1,181	0.250
Year 1	2,179	2,491	-312	0.157
Year 2	4,287	4,112	175	0.579
Year 3	5,486	4,760	726*	0.054
Year 4	5,638	5,046	592	0.131
Sample size	489	436		
Job-related				
Total earnings ($)				
Years 1-4	27,585	27,815	-230	0.933
Year 1	3,723	4,831	-1,108	0.101
Year 2	6,672	6,452	220	0.801
Year 3	8,442	8,188	254	0.806
Year 4	8,748	8,343	405	0.719
Sample size	91	106		
Other				
Total earnings ($)				
Years 1-4	13,871	14,449	-578	0.556
Year 1	1,611	2,156	-545***	0.005
Year 2	3,086	3,461	-375	0.192
Year 3	4,405	4,258	147	0.704
Year 4	4,770	4,574	196	0.617
Sample size	408	411		

(continued)

TABLE 5.3 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

a prior arrest than for other young out-of-school males, a finding that is consistent with the apparent pattern of worse impacts for male youths who face more barriers to employment.[3]

Comparing the earnings impacts for subgroups based on reasons for dropping out of high school, those for youths who had dropped out for school-related reasons were the best. These youths had the lowest initial earnings losses among the three subgroups, a statistically significant earnings impact of $726 in the third year of follow-up, and a four-year impact of $1,181. In contrast, those who had dropped out of high school to find a job (and whose control group counterparts were most likely to be working) had large earnings losses in the first year and relatively small gains in subsequent years, for a four-year impact that was slightly negative. Those who had dropped out for other reasons fared the worst over the four years: They had high initial earnings losses and the smallest earnings gains in years three and four.

II. The Timing of Payoffs to the Investment in JOBSTART

As is true of most investments, many of the costs of participating in a program such as JOBSTART are incurred in the short run. Any benefits will accrue over the rest of the participant's lifetime, if the new skills learned pay off in the labor market. Figure 5.1 presents a theoretical model for analyzing the labor market aspects of this investment and an alternative investment made by young people in the control group.

The first shaded area at the left of the figure represents foregone earnings sacrificed by program participants while they expend time and effort to improve their skills, in hope of a future payoff. These foregone earnings are an example of what economists term the "opportunity costs" of choosing one course of action over another.[4]

The figure also predicts that the control group's earnings will rise over time, as the group's employment rate rises with the group's growing maturity and experience and, for some members, with their acquisition of new skills on the job. For young people with poor skills, work experience can be an important source of new skills, which can translate into increased productivity and earnings and more stable employment. For their part, the young people in the experimental group can look for a job after their participation in JOBSTART ends, but their employment rates and earnings may not immediately exceed (or even reach) those of the controls who were already

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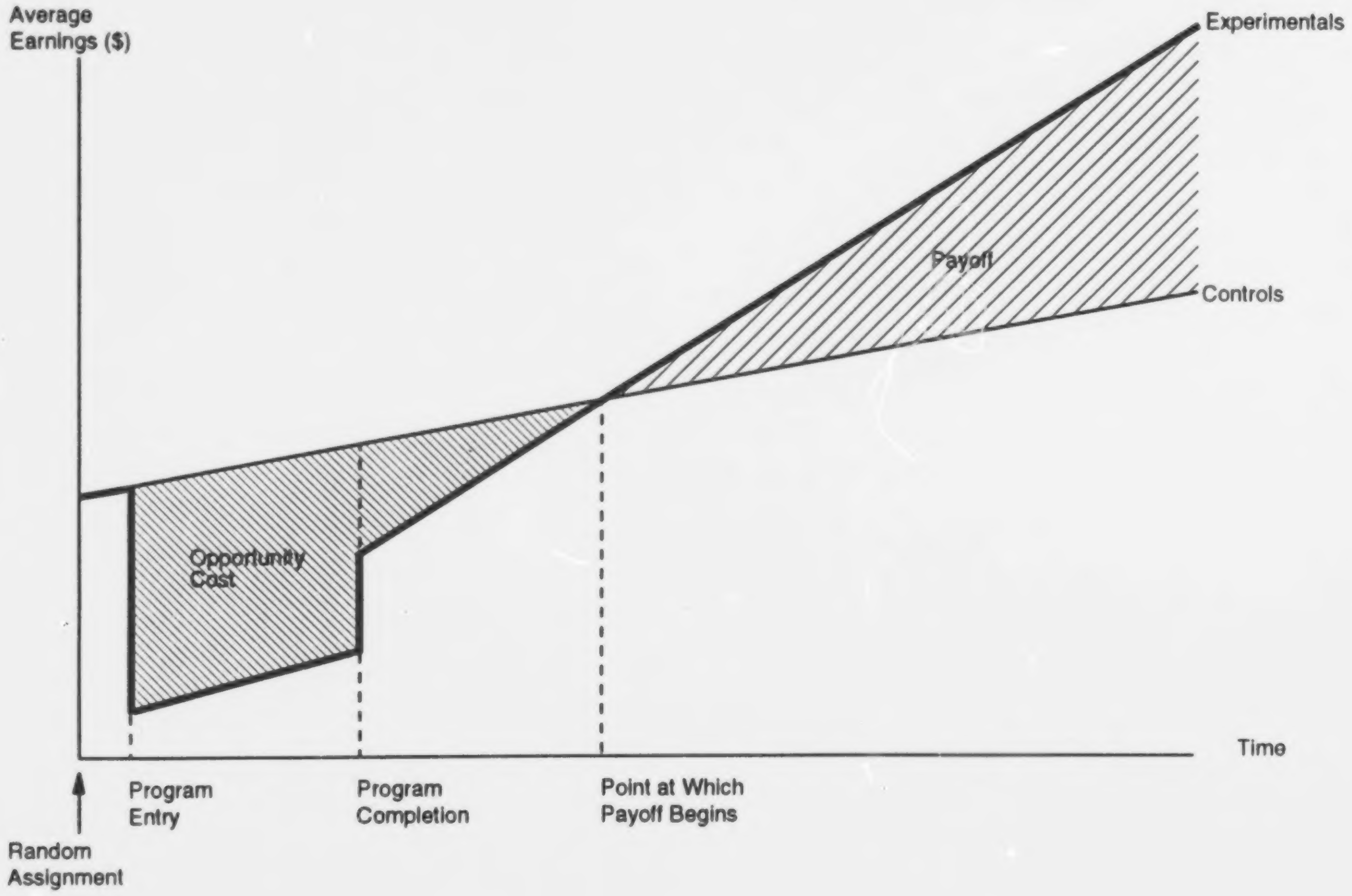
[3] See Bloom et al., 1993.

[4] In our example, these opportunity costs could also include other costs of program participation over and above the amount of foregone earnings.

FIGURE 5.1

A THEORETICAL VIEW OF THE PAYOFF OF A PERSONAL INVESTMENT IN EDUCATION AND TRAINING



working rather than attending a program. This post-program opportunity cost is the area in the figure between the end of program participation and the hoped-for point at which the earnings of the experimental group exceed those of the control group.

For a program such as JOBSTART to pay off for young people, the long-term benefits of increased education and training (represented in the figure by the shaded area on the right marked "payoff") must exceed the foregone – and more immediate – rewards of possible earnings and enhanced skills through work experience (represented in the figure as "opportunity costs"). Even in successful programs, it will take time for participants to overcome the head start of those who have been working throughout the program period. In other words, the "crossover" point (when experimentals start to earn more than controls) will not occur immediately.[5] Eventually, however, for the program to benefit participants, the amount of payoff must exceed the amount of opportunity cost (the shaded area on the right must become larger than the shaded area on the left).[6] Further, from a societal perspective, for a program to pass a test of economic efficiency, the benefits created by the program (for example, in the form of increased earnings) must exceed the resources used to provide program services.

The evaluation of the residential Job Corps program cited in Chapter 1 provides an example of an investment in education and training that was shown to benefit both participants and society.[7] That study began its follow-up at the point of termination from the program, and it reported impacts only for six-month intervals rather than for individual months. For the period from program termination to six months thereafter, the employment and earnings of those in the program group (participants) were slightly lower than those of the comparison group (although the differences were not statistically significant). The employment rates and earnings of the program group began to exceed those of the comparison group 6 to 12 months following program termination, and they continued to be higher throughout the remainder of the 48-month follow-up period. Moreover, the cumulative earnings gains the participants experienced over the follow-up period – the benefits – exceeded the costs of the program.[8]

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[5] A more detailed analysis of how JOBSTART affected the income of youths would include changes in other sources of income, such as public assistance, for experimentals and controls.

[6] As discussed in Chapter 7, benefits and costs must be discounted properly so they can be compared.

[7] See Mallar et al., 1982, p. 135.

[8] These findings are not directly applicable to the JOBSTART evaluation because follow-up in JOBSTART began at random assignment, the point when youths were ready to enter the program. However, since the average length of stay in JOBSTART was roughly six months, the 12-month survey was conducted about six months after the *average experimental group member* left the program, and the 24-month survey was conducted

(continued...)

The findings presented in Chapters 3 and 4 described the initial period of investment in JOBSTART services. During the first year after random assignment, JOBSTART experimentals had less time available for work than did their control group counterparts, because for much of that period they were in an intensive program of education and skills instruction. Chapter 3 showed that more than 50 percent of experimentals were still active in the sixth month after entering the sample; 15 percent were still active in the twelfth month; and almost 10 percent were still active in the fifteenth month. Throughout most of the first two years of follow-up, a larger percentage of experimentals than controls were participating in an education or training program. The controls, therefore, got a head start in the labor market, since the experimentals could not both be in JOBSTART and be working during any given hour of the day. (Again, for young people with poor skills, work experience itself can be an important source of new job skills and higher wages.) GED attainment during or after intensive JOBSTART education ultimately may open up many employment opportunities for JOBSTART graduates, but even after an experimental group member left JOBSTART, it still might have taken some time to become as well-settled in the labor market as his or her control group counterpart, who might have been learning new job skills while working. Thus, well into the second year of follow-up, controls still might be expected to have had the edge on experimentals in employment and earnings.

How well does the actual time path of earnings fit the theoretical model of investment and payoff? Figures 5.2a through 5.2d give the answer to this question by disaggregating the annual estimates of Tables 5.1 and 5.2 into monthly earnings for experimentals and controls. Figure 5.2a charts the progress of the full sample, while 5.2b through 5.2d present the story for custodial mothers, all other women, and men. Although none of the graphs look precisely like the theoretical model in Figure 5.1, there is a broad resemblance to the main features of the model in each of the four cases: a period of investment followed by a payback period. The point at which the curve for experimentals crosses above the curve for controls is near month 23 for the full impact sample, month 24 for the subgroup of men, month 25 for custodial mothers, and month 20 for other women.

In the discussion of JOBSTART's benefits and costs in Chapter 7, benefits other than earnings gains are also considered. But this examination of trends suggests that JOBSTART's impacts on earnings pass the first test for a successful investment: A payoff seems to have begun for many key

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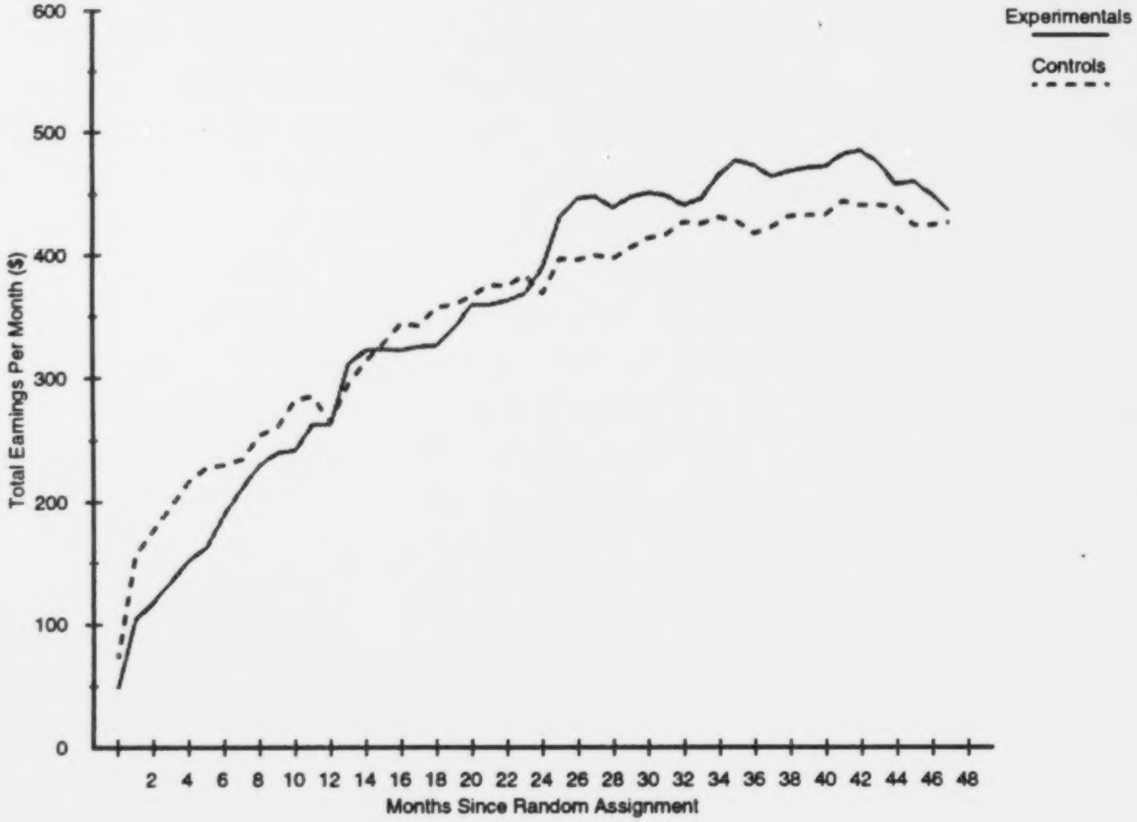
[8](...continued)
about 18 months after that average experimental left the program. Thus, the semi-annual earnings and employment impacts in the Job Corps study turned positive at a point roughly corresponding to the year-two estimates in this report.

FIGURE 5.2a

MONTHLY EARNINGS FOR THE FULL SAMPLE, BY RESEARCH STATUS



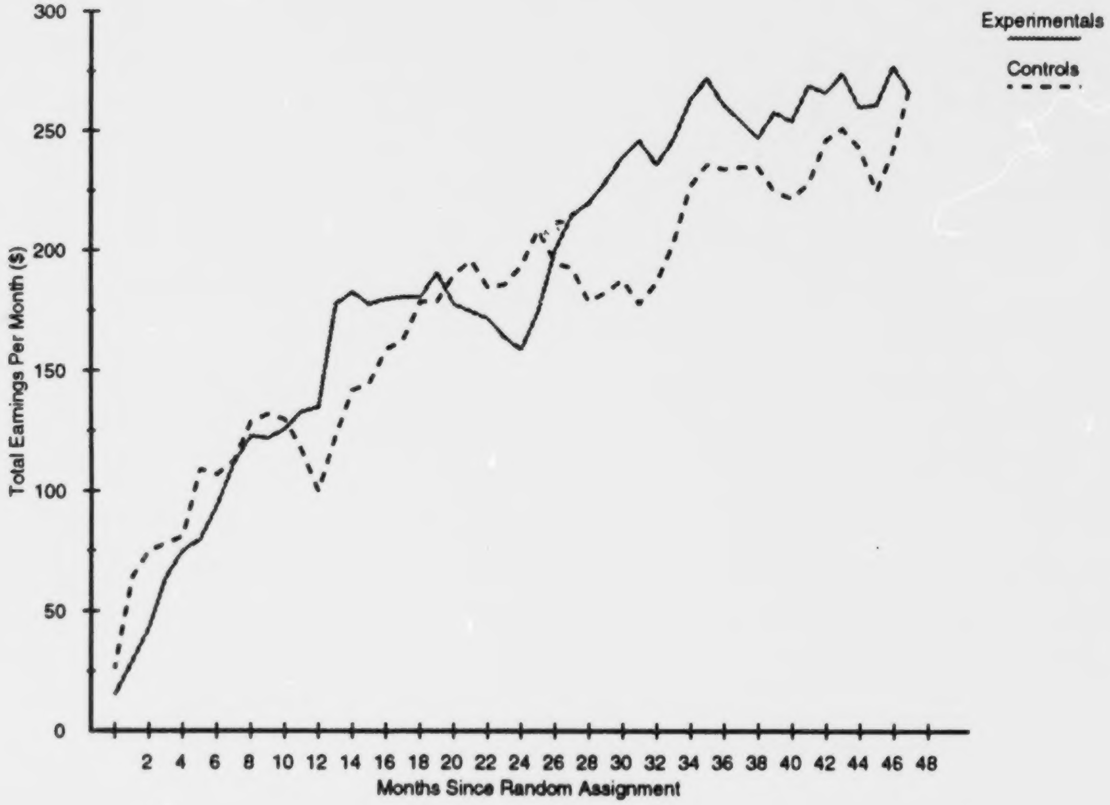
SOURCE: Appendix Table D.1.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

FIGURE 5.2b

MONTHLY EARNINGS FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS



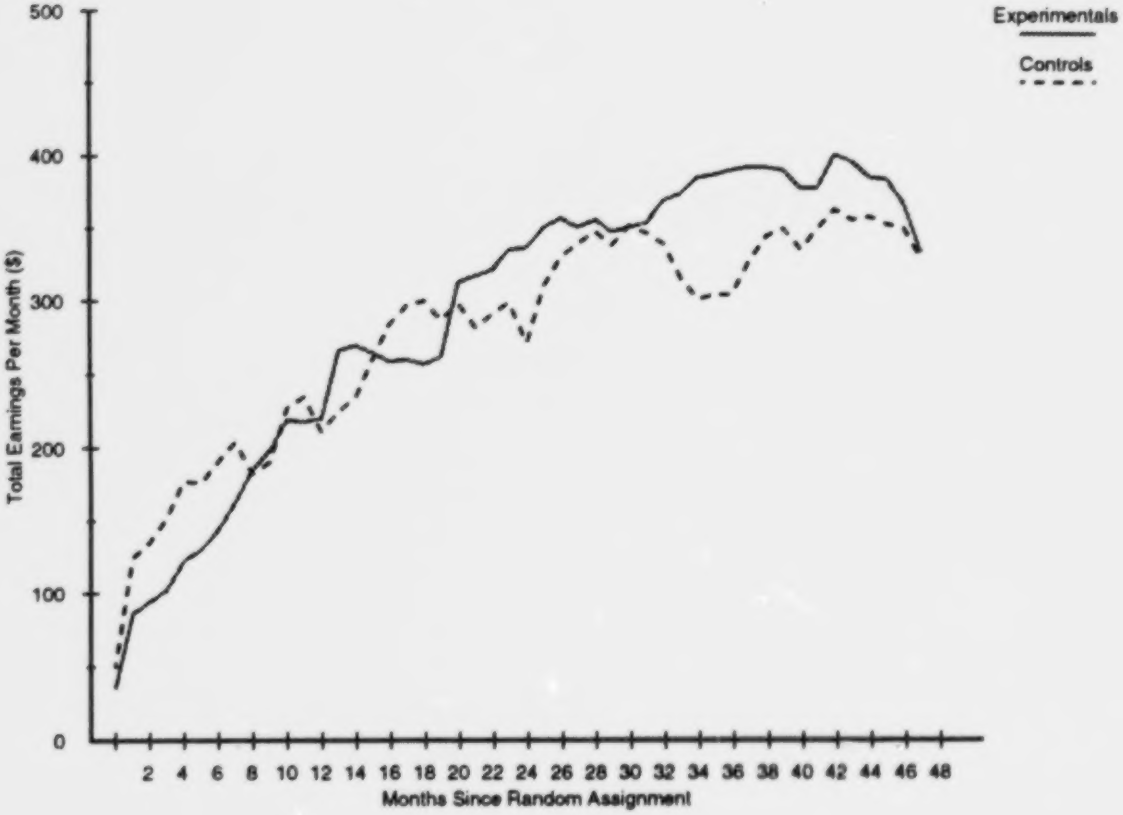
SOURCE: Appendix Table D.2.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

FIGURE 5.2c

MONTHLY EARNINGS FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS [a]



SOURCE: Appendix Table D.3.

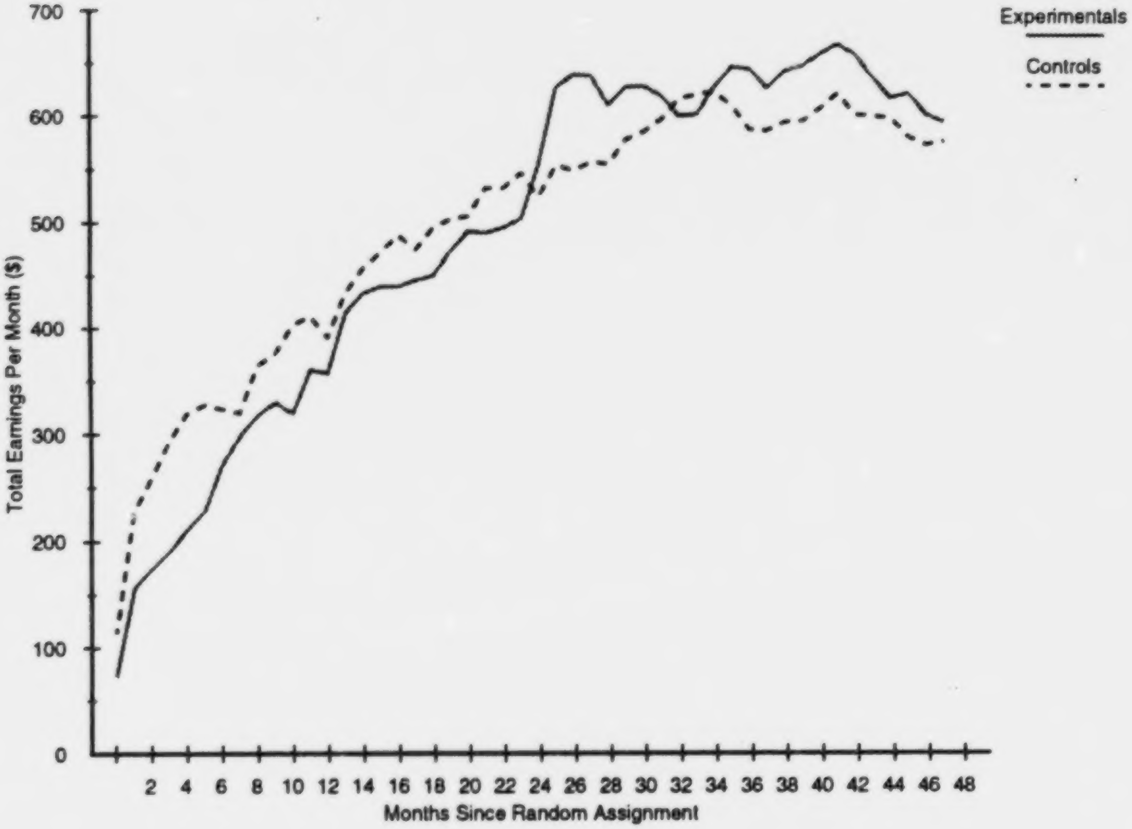
NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

[a] Includes women who did not have children.

FIGURE 5.2d

MONTHLY EARNINGS FOR MEN, BY RESEARCH STATUS



SOURCE: Appendix Table D.4.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

subgroups before the end of the follow-up period. Moreover, Tables 5.2 and 5.3 show that for some subgroups – custodial mothers, other women, males arrested since age 16, and youths who dropped out of school for school-related reasons – JOBSTART passes the second test as of the four-year point: Earnings gains in the later months of follow-up outweighed the initial foregone earnings so that the experimentals appeared to earn more than the controls (although the four-year impacts were not statistically significant). Chapter 7 discusses whether JOBSTART passes another efficiency test by having a sufficiently large excess of payoffs (both in earnings gains and other benefits) to cover direct program and other costs.

III. Impacts on Employment Rates, Hours of Work, and Weeks of Work

A different kind of analysis of earnings entails looking beyond time paths of monthly earnings to the impacts of JOBSTART on the components of earnings. More specifically, earnings are the product of hours worked and wages per hour. Thus, any percentage change in earnings is approximately equal to the sum of a percentage change in hours of work and a percentage change in earnings per hour. Moreover, hours of work can change for a number of reasons, including changed rates of employment, changed hours of work per week, and changed weeks of work per period. Analyzing these more refined measures of labor market success helps explain the basic findings on earnings impacts. This section examines differences between experimentals and controls in the time spent working; the following section will discuss differences in wages between those experimentals and controls who worked.[9]

As expected, controls were more likely to work than experimentals in the first year of follow-up. In the second year, despite a slightly higher employment rate for experimentals (71 percent) than for controls (67.5 percent), experimentals were not ahead in actual time spent working, although there were differences among the key subgroups. During the latter part of the four-year follow-up, experimentals began to receive the payoff for their earlier investment in human capital. This and the next section cover these more refined measures of investment and the beginning of the payoff for the full sample and each of the three major subgroups.

Because young people tend to apply for employment and training programs when they are between jobs or about to enter or reenter the labor force, the trend in the monthly employment rate was upward during the first two years for both experimentals and controls (Figure 5.3a). For the full sample, a larger fraction of controls than experimentals was employed in each month of the first year. The difference in employment rates peaked in months four and five and then narrowed. After month 12, the average employment rate of experimentals was greater than that of controls more often than it was below it, but in only one month was the difference statistically significant. During the last two years of follow-up, monthly employment rates leveled off in the middle to high 40 percent range, with experimentals mainly employed at monthly rates insignificantly higher than those for controls (only during month 37 was the difference statistically significant). Figures 5.3b and 5.3c show that the average number of hours and weeks worked per month followed roughly the same pattern as employment rates.

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[9] The separate presentations are meant to highlight an important methodological distinction: The impacts on employment rates, hours worked, and weeks worked are estimated experimentally, whereas the impacts on wage rates for those who worked are estimated using nonexperimental techniques.

Table 5.4 aggregates the monthly results in Figure 5.3a into cumulative and annual outcomes and impacts for the full sample. Over the four-year period as a whole there was almost no experimental-control difference in the percentage employed at some time: 86.4 percent of experimentals and 86 percent of controls were employed at some point. The modest 1.3 percent gain in four-year earnings therefore must have come mainly from greater earnings per hour for experimentals (as discussed in Section IV below), since it does not seem to have stemmed from greater employment rates or more hours or weeks of work. The year-by-year pattern is instructive as well. The -$499 (19 percent) loss in year-one earnings stemmed from negative program impacts on all three employment measures: the employment rate and weeks and hours worked. During year two, the employment rate of experimentals exceeded that of controls, the disparity in weeks of work disappeared, and the hours gap narrowed to a very small amount, but not enough to yield earnings gains during the second year. During the last two years the earnings gains were mainly the result of more hours and weeks worked by experimentals and not of a difference in employment rates.

A. Impacts for Custodial Mothers

As explained in Chapter 4, many previous evaluations of employment and training programs for youths have found better program effects for young women than for young men.[10] One likely explanation for women's better employment results is that it is easier to improve the employment and earnings of those who do not spend much time in the world of work (for example, young mothers) than of those who are already in the labor force but fail to find and keep steady, well-paying jobs (for example, young men with poor skills). Thus, from this perspective, young

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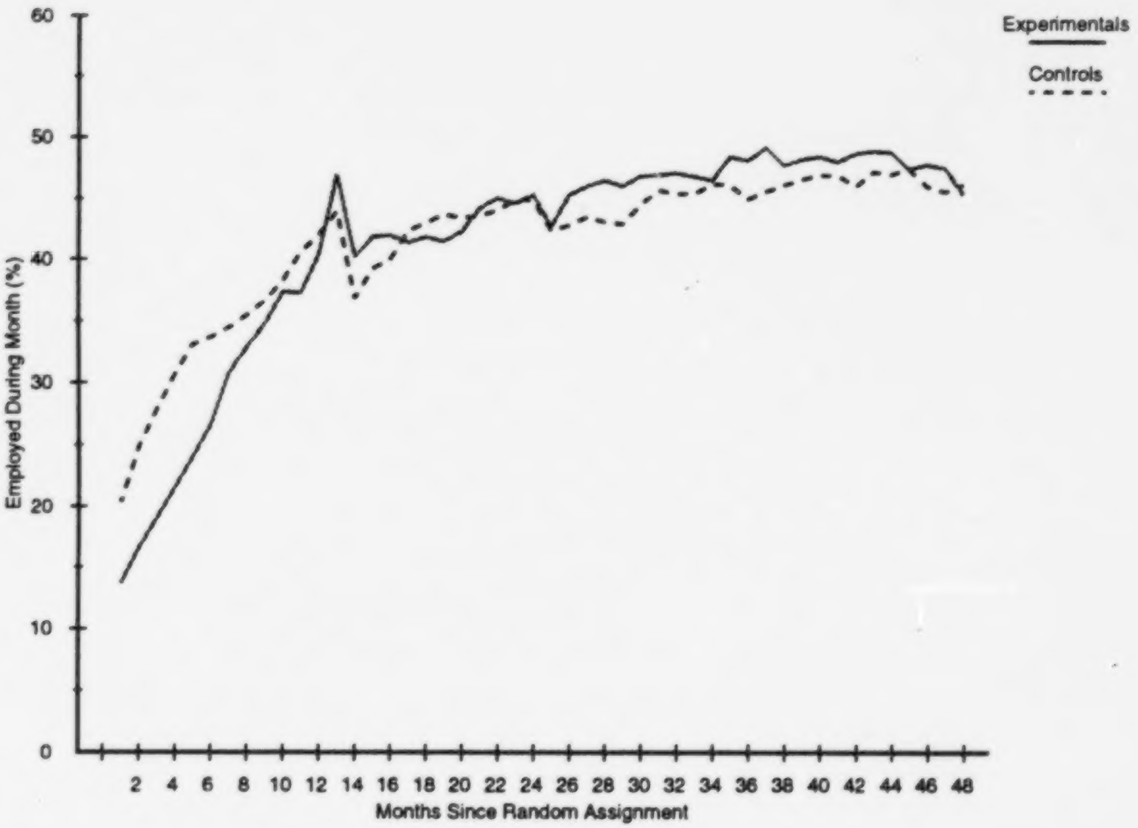
[10] For a comprehensive review of the studies, see Betsey et al., 1985.

FIGURE 5.3a

MONTHLY EMPLOYMENT RATES FOR THE FULL SAMPLE, BY RESEARCH STATUS



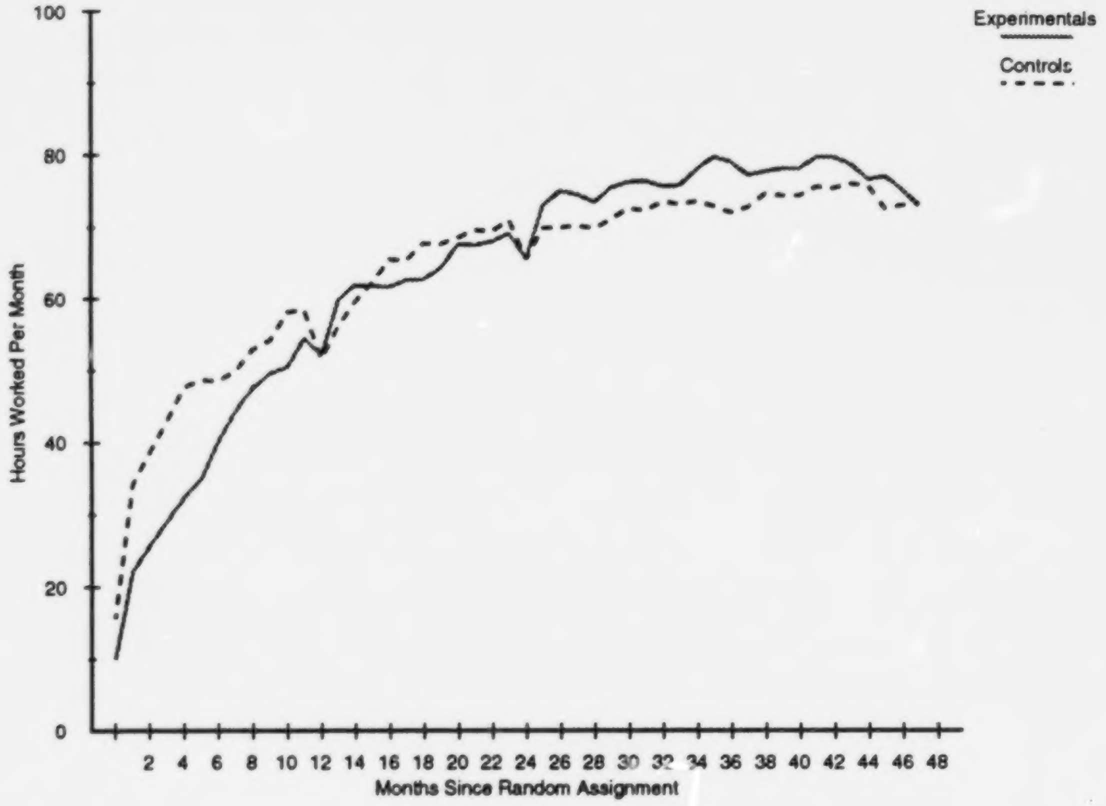
SOURCE: Appendix Table D.5.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

FIGURE 5.3b

MONTHLY HOURS WORKED FOR THE FULL SAMPLE, BY RESEARCH STATUS



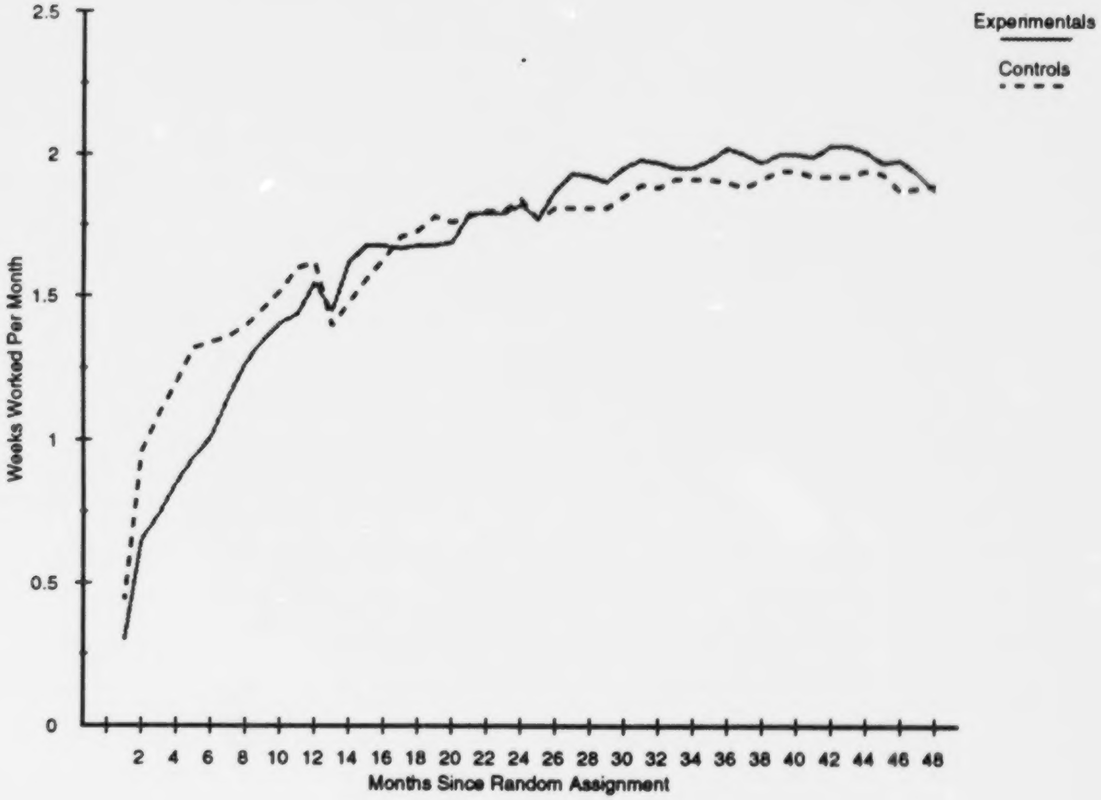
SOURCE: Appendix Table D.6.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

FIGURE 5.3c

MONTHLY WEEKS WORKED FOR THE FULL SAMPLE, BY RESEARCH STATUS



SOURCE: Appendix Table D.7.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

TABLE 5.4

IMPACTS ON ANNUAL EARNINGS, EMPLOYMENT RATES, HOURS OF WORK, AND WEEKS OF WORK THROUGH MONTH 48

Follow-Up Period and Outcome	Experimentals	Controls	Difference	p[a]
Years 1-4				
Total earnings ($)	17,010	16,796	214	0.757
Ever employed (%)	86.4	86.0	0.4	0.789
Total hours worked	3,030.57	3,070.69	-40.12	0.700
Total weeks employed	79.96	80.81	-0.84	0.741
Year 1				
Total earnings ($)	2,097	2,596	-499***	0.001
Ever employed (%)	56.5	60.8	-4.3**	0.041
Total hours worked	441.08	550.13	-109.05***	0.000
Total weeks employed	12.67	15.30	-2.63***	0.000
Year 2				
Total earnings ($)	3,991	4,112	-121	0.563
Ever employed (%)	71.0	67.5	3.5*	0.073
Total hours worked	760.23	775.30	-15.06	0.660
Total weeks employed	20.32	20.27	0.05	0.953
Year 3				
Total earnings ($)	5,329	4,906	423	0.102
Ever employed (%)	61.8	61.5	0.3	0.865
Total hours worked	898.96	854.83	44.12	0.249
Total weeks employed	23.20	22.28	0.92	0.323
Year 4				
Total earnings ($)	5,592	5,182	410	0.125
Ever employed (%)	65.7	64.5	1.3	0.536
Total hours worked	930.30	890.42	39.88	0.301
Total weeks employed	23.77	22.95	0.82	0.370
Sample size	988	953		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

women have greater potential for improved labor market outcomes than young men, and less to lose (in terms of foregone employment and earnings) by investing in education and training. The JOBSTART sample exemplifies this pattern.[11]

We begin with the findings for custodial mothers. Figures 5.4a-c display experimental-control differences in the monthly time paths of employment rates, hours worked, and weeks worked for these women. The graphs each show roughly the same time trend as the trend in monthly employment rates presented earlier in Figure 5.3a. Table 5.5 aggregates the monthly results for custodial mothers from Figures 5.4a-c into annual outcomes and impacts. As shown in the top panel, there was a statistically insignificant difference of 4.5 percentage points in the proportion who had any paid employment during the four-year follow-up period – 75.4 percent of experimentals versus 71 percent of controls. Thus, the $625 (7.5 percent) gain in four-year earnings for custodial mothers may have come primarily from higher employment rates for experimentals than for controls, since the overall gains in hours and weeks of work over the four years were noticeably smaller than 7.5 percent. The year-by-year pattern in the last four panels shows a small but statistically insignificant gain in the employment rate even in the first year, a relatively large and significant employment rate gain during the second year, but a narrowing of the gap to virtually zero by the fourth year as the control group employment rate caught up and the experimental rate leveled off. In other words, by the fourth year, custodial mothers in the experimental group earned more than their control group counterparts primarily because they worked for more hours and weeks, not because they had higher employment rates or earnings per hour (as explained further in Section IV below).

B. Impacts for Other Women

Figures 5.5a-c show monthly time paths of employment rates, hours worked, and weeks worked for women who were not custodial mothers at baseline. The pattern here is quite different from that for custodial mothers. Employment rate differences seem to have been more important for other women than for custodial mothers, especially toward the beginning of follow-up (when controls worked more than experimentals) and toward the end (when experimentals moved ahead of controls in most months). When considering the control group's employment rates for other women in the early months of follow-up, it seems that experimentals sacrificed more hours and

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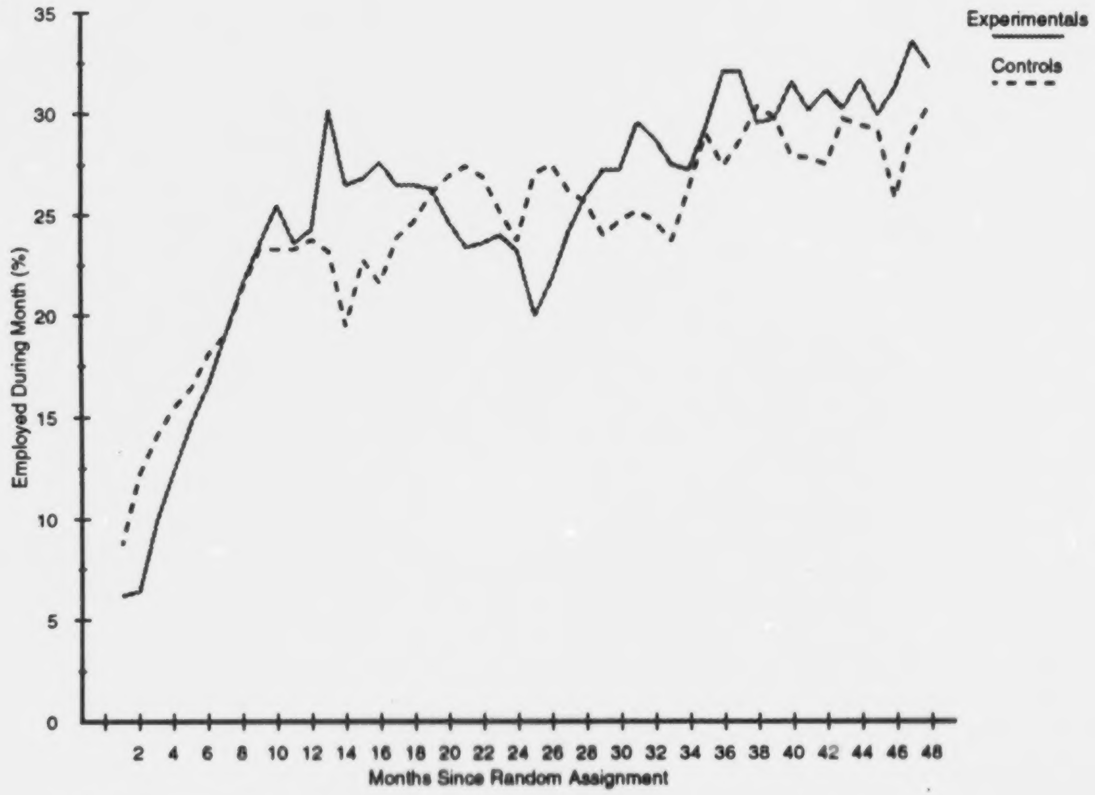
[11] Few of the impact estimates reported in this section are statistically significant, in part because of the relatively small sample size of the subgroups examined.

FIGURE 5.4a

MONTHLY EMPLOYMENT RATES FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS



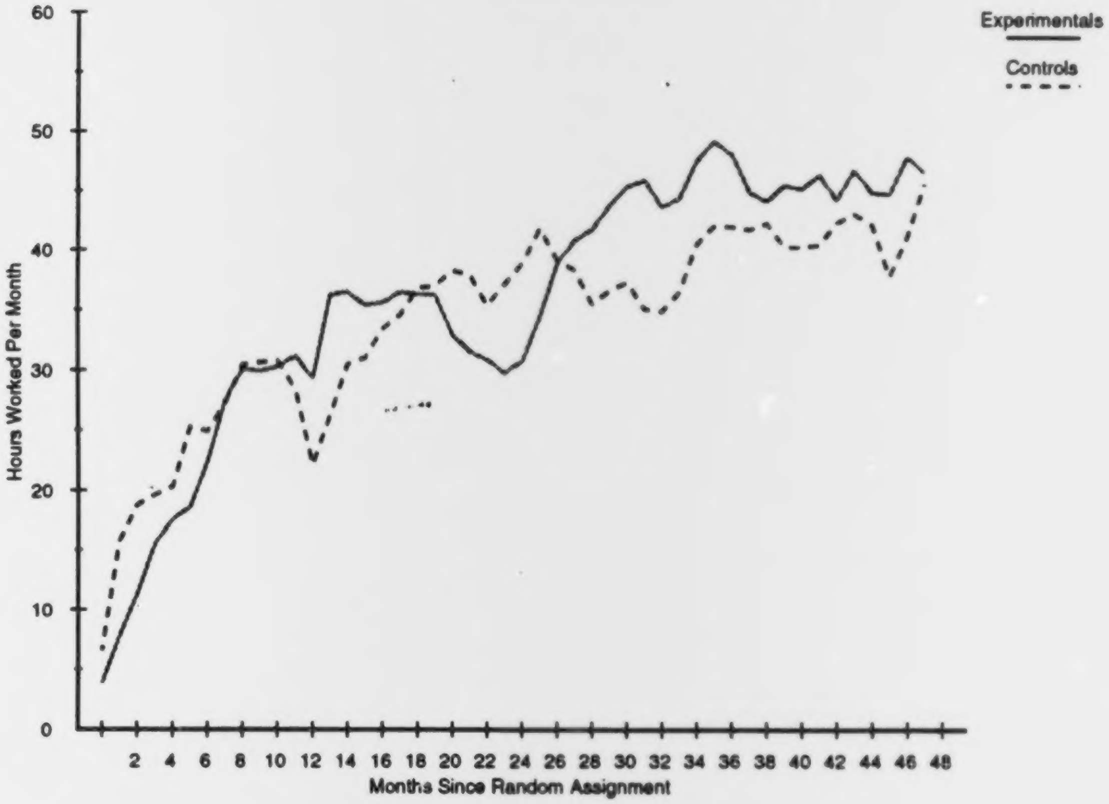
SOURCE: Appendix Table D.8.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

FIGURE 5.4b

MONTHLY HOURS WORKED FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS



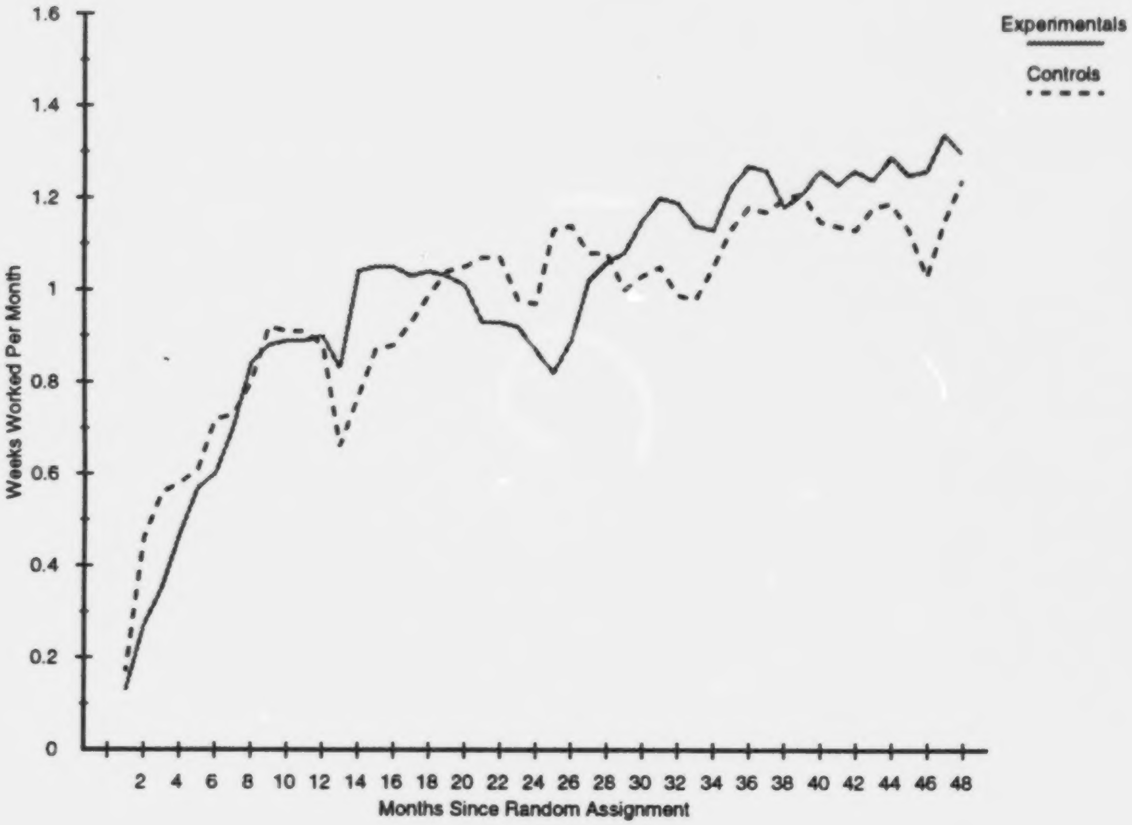
SOURCE: Appendix Table D.9.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

FIGURE 5.4c

MONTHLY WEEKS WORKED FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS



SOURCE: Appendix Table D.10.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

TABLE 5.5

IMPACTS ON ANNUAL EARNINGS, EMPLOYMENT RATES, HOURS OF WORK, AND WEEKS OF WORK THROUGH MONTH 48, FOR WOMEN LIVING WITH THEIR OWN CHILD(REN) AT THE TIME OF RANDOM ASSIGNMENT

Follow-Up Period and Outcome	Experimentals	Controls	Difference	p[a]
Years 1-4				
Total earnings ($)	8,959	8,334	625	0.557
Ever employed (%)	75.4	71.0	4.5	0.257
Total hours worked	1,708.78	1,635.57	73.21	0.678
Total weeks employed	47.45	46.29	1.16	0.798
Year 1				
Total earnings ($)	1,016	1,160	-144	0.451
Ever employed (%)	41.0	38.8	2.2	0.616
Total hours worked	246.27	279.00	-32.73	0.456
Total weeks employed	7.49	8.26	-0.77	0.511
Year 2				
Total earnings ($)	2,097	1,947	150	0.648
Ever employed (%)	53.2	45.5	7.8*	0.078
Total hours worked	408.09	401.15	6.94	0.902
Total weeks employed	11.72	11.26	0.46	0.758
Year 3				
Total earnings ($)	2,700	2,372	328	0.409
Ever employed (%)	42.7	41.2	1.5	0.740
Total hours worked	505.96	456.54	49.42	0.456
Total weeks employed	13.17	12.85	0.32	0.851
Year 4				
Total earnings ($)	3,146	2,856	290	0.505
Ever employed (%)	49.1	49.3	-0.2	0.957
Total hours worked	548.46	498.87	49.59	0.453
Total weeks employed	15.08	13.92	1.16	0.490
Sample size	257	251		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

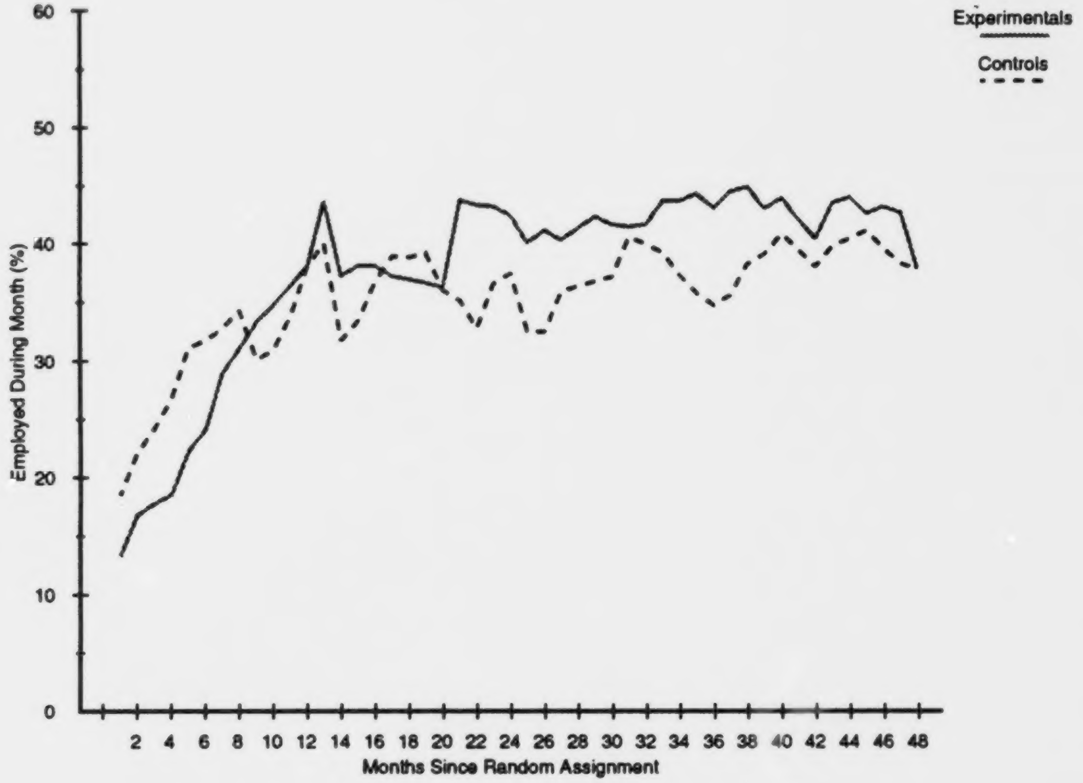
Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 28 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

FIGURE 5.5a

MONTHLY EMPLOYMENT RATES FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS [a]



SOURCE: Appendix Table D.11.

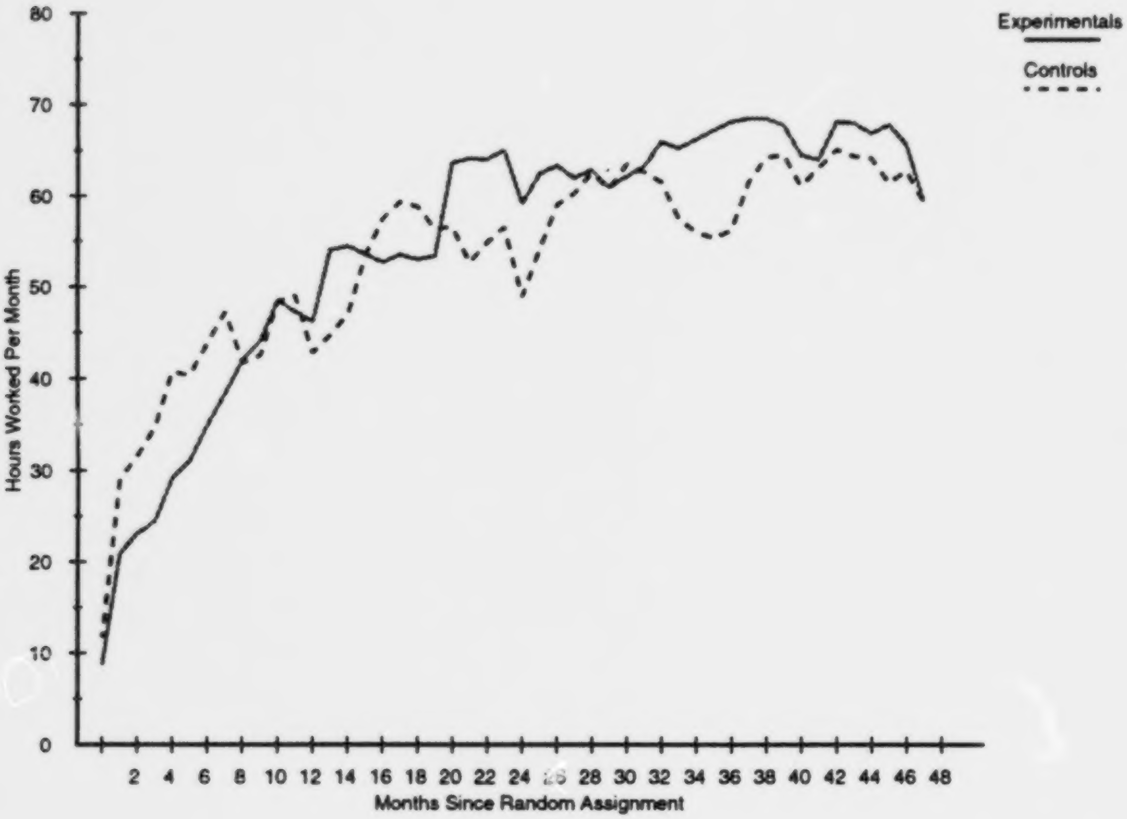
NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

[a] Includes women who did not have children.

FIGURE 5.5b

MONTHLY HOURS WORKED FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS [a]



SOURCE: Appendix Table D.12.

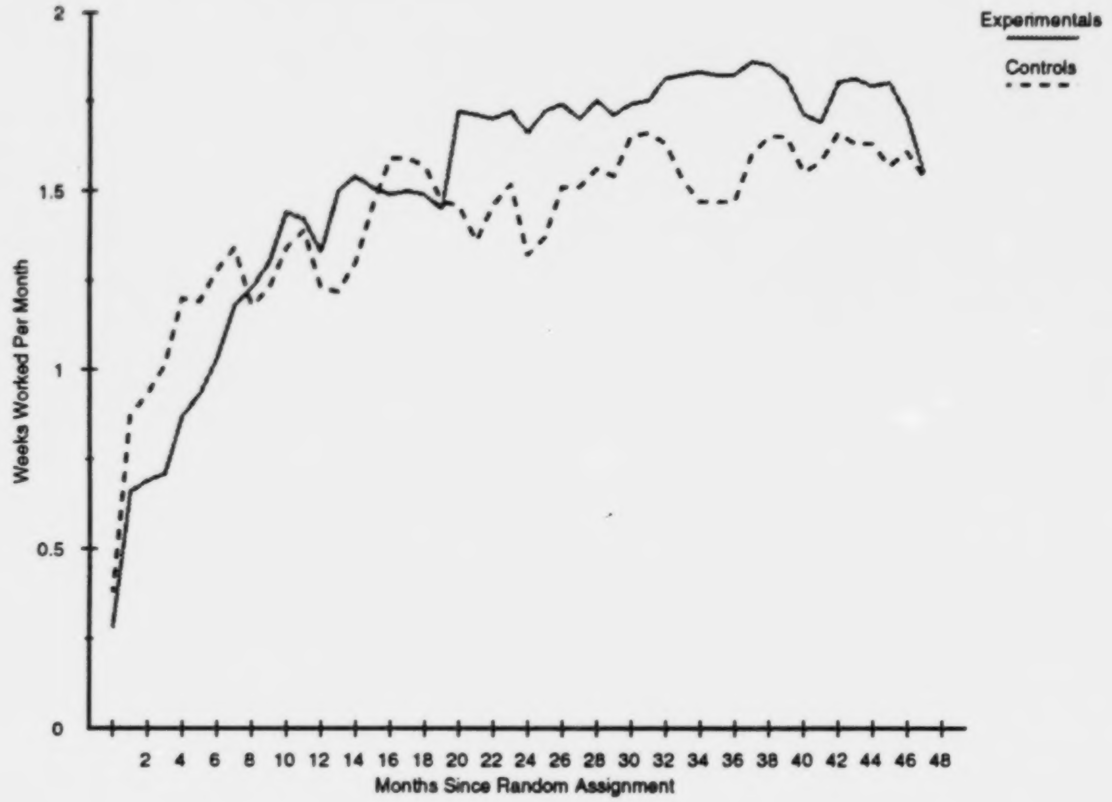
NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

[a] Includes women who did not have children.

FIGURE 5.5c

MONTHLY WEEKS WORKED FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS[a]



SOURCE: Appendix Table D.13.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

[a] Includes women who did not have children.

weeks of work to take part in the program than did their custodial mother counterparts, but they overtook the controls in these measures in later months, during the post-program period.

Table 5.6 aggregates the other women's monthly results from Figures 5.5a-c into annual outcomes and impacts. Over the full four-year period, there was no gain in employment rates – 84.3 percent of experimentals and 85.3 percent of controls were employed at some point. While the dollar amount of earnings gains over the four years for other women was similar in magnitude to that for custodial mothers, it was a smaller percentage; in the control group, custodial mothers earned an average of $8,334 over four years, while other women earned an average of $13,310. In the third and fourth years of follow-up, gains in weeks of work seem important in explaining earnings gains for other women.[12]

C. Impacts for Men

Figures 5.6a-c show much less of a difference over time in the monthly employment measures of male experimentals and controls than was the case for either custodial mothers or other women. After initial negative impacts on employment rates, hours worked, and weeks worked early in the follow-up period (when most experimentals were participating in JOBSTART), no positive and statistically significant monthly impacts on any of these outcomes appeared for men. This was in contrast to the trend for women who were not custodial mothers, for whom positive impacts did appear in the later months of follow-up.

Table 5.7 aggregates the monthly results in Figures 5.6a-c into annual outcomes and impacts for young men. Over the full four-year period, almost all men worked at some point, and there was almost no difference between experimentals and controls in the percentage who ever worked – 94.1 percent versus 94.5 percent. Behind the modest $273 (1.2 percent) loss in four-year earnings are larger percentage declines in hours and weeks of work. Average earnings per hour worked appear to have been higher for experimentals, thereby partially offsetting the negative impacts on hours and weeks of work. This issue is explored in more detail in the next section.

During the first year after random assignment, 74.9 percent of male controls worked, compared to 38.8 percent of control group custodial mothers and 57.8 percent of other women. During this period, which for many experimentals was largely a time of program participation, the impact of JOBSTART on employment rates was 2.2 percentage points for custodial mothers, -2.2 percentage

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[12] Section IV presents nonexperimental estimates of impacts on hourly wages that suggest that wage gains were also important in the third and, especially, fourth years.

TABLE 5.6

IMPACTS ON ANNUAL EARNINGS, EMPLOYMENT RATES, HOURS OF WORK, AND WEEKS OF WORK THROUGH MONTH 48, FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN) AT THE TIME OF RANDOM ASSIGNMENT

Follow-Up Period and Outcome	Experimentals	Controls	Difference	p[a]
Years 1-4				
Total earnings ($)	13,923	13,310	613	0.609
Ever employed (%)	84.3	85.3	-1.0	0.746
Total hours worked	2,628.16	2,552.21	75.95	0.704
Total weeks employed	72.64	67.94	4.70	0.361
Year 1				
Total earnings ($)	1,697	2,040	-343	0.126
Ever employed (%)	55.6	57.8	-2.2	0.604
Total hours worked	393.34	461.29	-67.94	0.158
Total weeks employed	11.75	13.35	-1.60	0.232
Year 2				
Total earnings ($)	3,345	3,269	76	0.831
Ever employed (%)	68.5	62.4	6.1	0.138
Total hours worked	678.02	641.27	36.75	0.571
Total weeks employed	18.65	17.23	1.42	0.395
Year 3				
Total earnings ($)	4,309	3,889	420	0.360
Ever employed (%)	55.4	54.5	0.8	0.850
Total hours worked	759.97	702.06	57.91	0.438
Total weeks employed	21.05	18.21	2.84	0.136
Year 4				
Total earnings ($)	4,572	4,111	461	0.342
Ever employed (%)	60.7	57.2	3.5	0.406
Total hours worked	796.83	747.60	49.24	0.512
Total weeks employed	21.19	19.15	2.04	0.268
Sample size	283	250		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

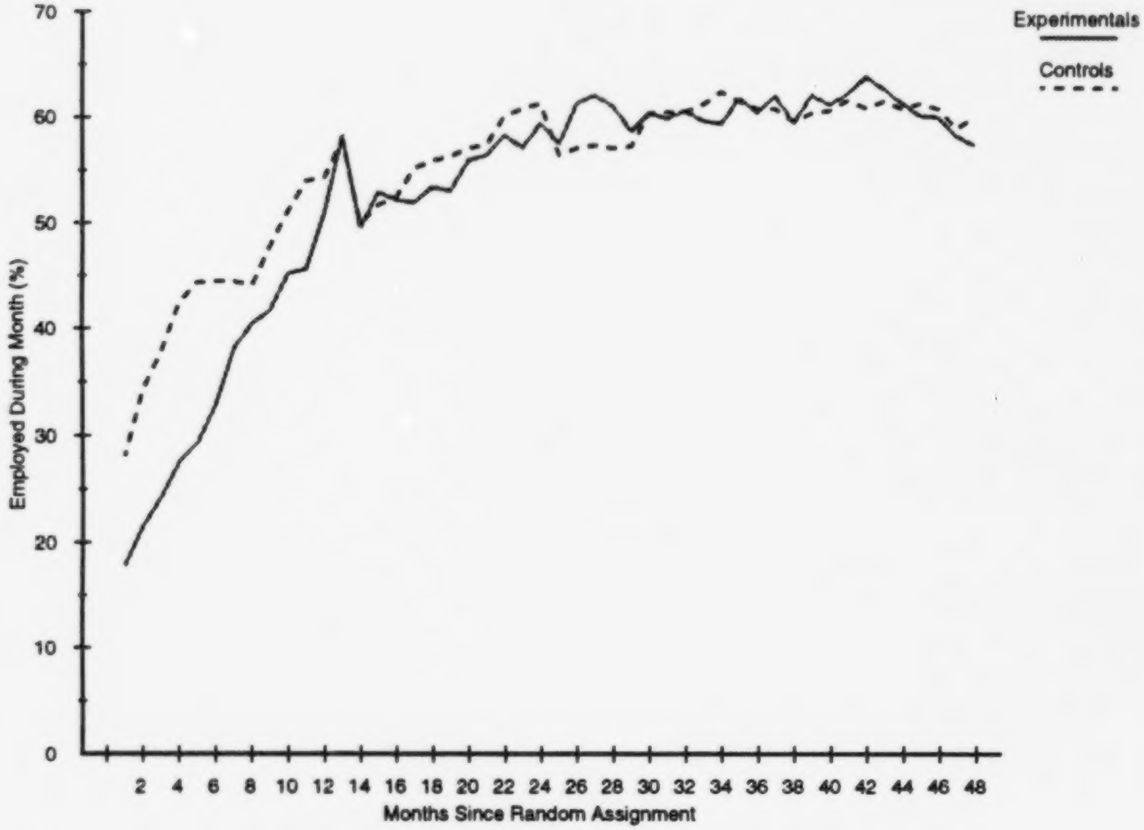
Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 28 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

FIGURE 5.6a

MONTHLY EMPLOYMENT RATES FOR MEN, BY RESEARCH STATUS



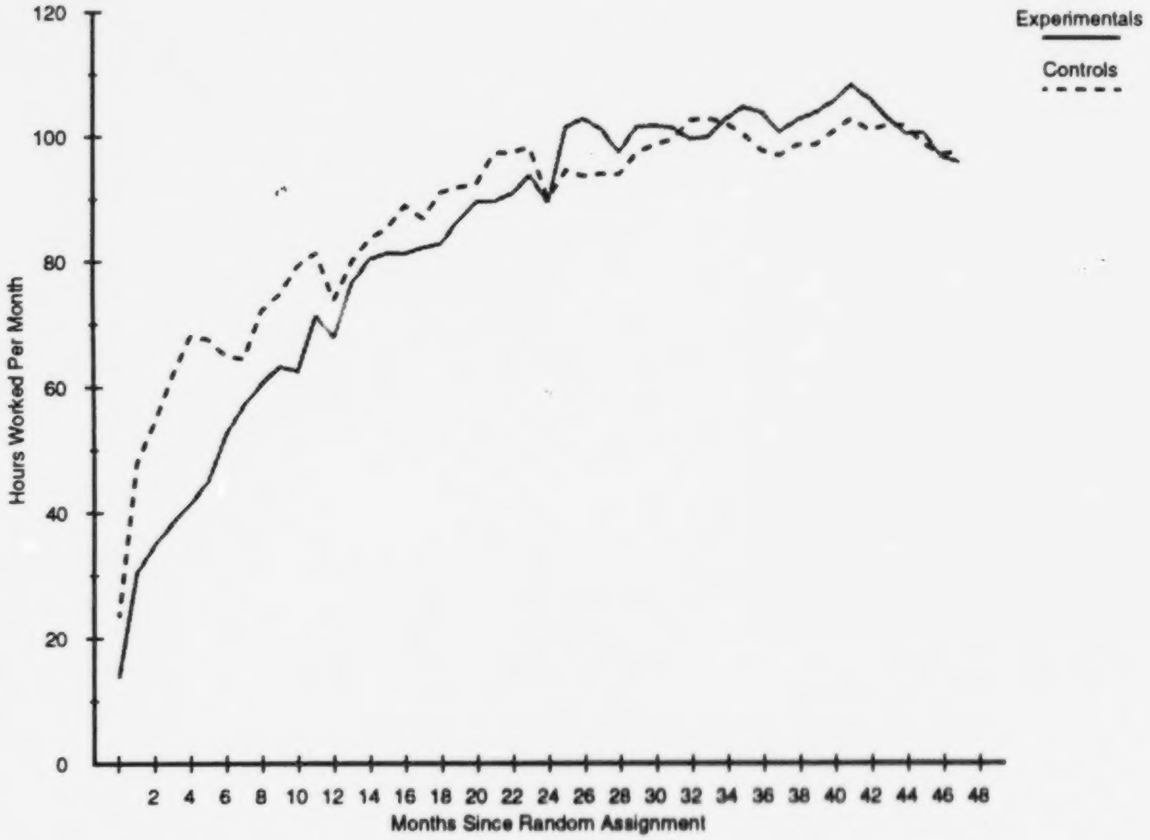
SOURCE: Appendix Table D.14.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

FIGURE 5.6b

MONTHLY HOURS WORKED FOR MEN, BY RESEARCH STATUS



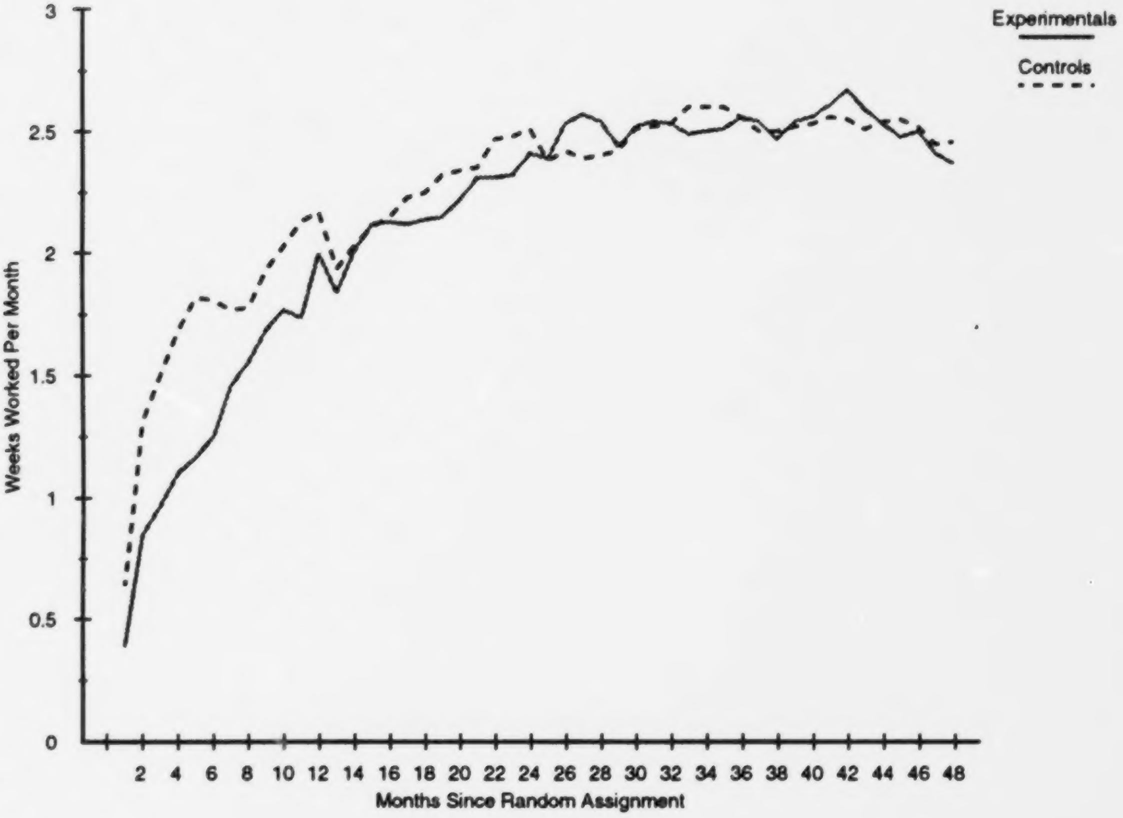
SOURCE: Appendix Table D.15.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

FIGURE 5.6c

MONTHLY WEEKS WORKED FOR MEN, BY RESEARCH STATUS



SOURCE: Appendix Table D.16.

NOTES: Calculations for this figure used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

TABLE 5.7

IMPACTS ON ANNUAL EARNINGS, EMPLOYMENT RATES, HOURS OF WORK, AND WEEKS OF WORK THROUGH MONTH 48, FOR MEN

Follow-Up Period and Outcome	Experimentals	Controls	Difference	p[a]
Years 1-4				
Total earnings ($)	23,364	23,637	-273	0.818
Ever employed (%)	94.1	94.5	-0.4	0.803
Total hours worked	4,006.04	4,191.04	-185	0.267
Total weeks employed	102.44	107.89	-5.44	0.164
Year 1				
Total earnings ($)	2,929	3,741	-812***	0.003
Ever employed (%)	65.7	74.9	-9.2***	0.002
Total hours worked	571.90	760.84	-188.95***	0.000
Total weeks employed	15.96	20.56	-4.60***	0.000
Year 2				
Total earnings ($)	5,435	5,831	-396	0.266
Ever employed (%)	83.0	82.3	0.7	0.772
Total hours worked	1,003.82	1,067.47	-63.65	0.250
Total weeks employed	26.08	27.19	-1.11	0.397
Year 3				
Total earnings ($)	7,401	6,957	444	0.320
Ever employed (%)	76.6	76.9	-0.3	0.916
Total hours worked	1,203.34	1,169.29	34.06	0.574
Total weeks employed	30.14	29.95	0.19	0.894
Year 4				
Total earnings ($)	7,599	7,107	492	0.271
Ever employed (%)	78.6	76.7	2.0	0.468
Total hours worked	1,226.99	1,193.44	33.55	0.583
Total weeks employed	30.26	30.19	0.07	0.957
Sample size	448	452		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

points for other women, and a statistically significant -9.2 percentage points for men. Again, these findings underscore how, even more for young men than for other women, JOBSTART entailed a substantial opportunity cost in foregone employment that custodial mothers did not have to pay. Even after four years, the hours and weeks of work forfeited during program participation had not been recouped in post-program gains. Clearly, this high opportunity cost is a primary cause of the poor results for men.

A second possible explanation for the poorer impacts observed for men in JOBSTART is the greater difficulty of placing them in jobs that value a GED. Many women in JOBSTART, for example, were trained in clerical occupations and sought clerical jobs, for which educational credentials were important. However, young men were more likely to train for work that did not, at least initially, value a GED – for example, in many types of blue-collar occupations, especially physically demanding jobs.[13] Further, it may have been harder to find training-related jobs for men who did study for occupations for which a GED mattered.

IV. Impacts on Earnings per Hour

Earnings are the product of hours spent working and wages earned per hour. Departing from the methodology of the analysis so far, the discussion in this section excludes those experimentals and controls who did not work during the periods indicated and therefore had no earnings. Thus, the experimental-control differences presented here are not experimentally estimated impacts, but they do help to understand the underlying factors of the impact findings.[14] The focus continues to be on the three key subgroups: men, custodial mothers, and all other women.

For each of these subgroups, both experimentals and controls generally showed gradual growth in earnings per hour over time, although the trends are not always clear. Table 5.8 shows the estimated hourly wages for working experimentals and controls for the three key subgroups. For custodial mothers, shown in the top panel of the table, estimated hourly wages for working experimentals were $4.14 in the first year and grew to about $5.45 by the fourth year; for working controls, hourly wages began at $4.00 and grew to $5.37. The differences in hourly wages in each year were relatively small, and during the entire four-year period, the average hourly wage for

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[13] Chapter 3 reported on occupations for which men and women trained and showed that a much higher percentage of women studied for clerical occupations.

[14] The experimental-control differences in this section were calculated using the Heckman correction for selection bias to account for the exclusion of experimentals and controls who did not work. See Heckman, 1979.

TABLE 5.8

ESTIMATES OF EFFECTS ON AVERAGE HOURLY WAGES THROUGH MONTH 48, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Sample Size (Workers Only)		Outcome			
	Experimentals	Controls	Experimentals	Controls	Difference	p[a]
Women living with own child(ren)						
Average hourly wages ($)						
Years 1-4	194	178	4.72	4.84	-0.12	0.524
Year 1	107	96	4.14	4.00	0.14	0.572
Year 2	137	114	4.63	4.55	0.08	0.850
Year 3	109	104	5.10	5.09	0.01	0.978
Year 4	125	125	5.45	5.37	0.08	0.767
Sample size (including nonworkers)	257	251				
Women not living with own child(ren), including those who did not have any						
Average hourly wages ($)						
Years 1-4	239	213	4.90	4.82	0.08	0.526
Year 1	158	144	4.14	4.43	-0.29*	0.082
Year 2	193	156	4.55	4.91	-0.36	0.111
Year 3	156	137	5.54	5.20	0.34	0.140
Year 4	171	144	5.70	4.93	0.77***	0.008
Sample size (including nonworkers)	283	250				
Men						
Average hourly wages ($)						
Years 1-4	421	428	5.45	5.36	0.09	0.438
Year 1	293	339	4.91	4.69	0.22	0.421
Year 2	369	373	5.13	5.20	-0.07	0.604
Year 3	343	348	5.86	5.83	0.03	0.869
Year 4	352	347	5.89	5.82	0.07	0.643
Sample size (including nonworkers)	448	452				

(continued)

TABLE 5.8 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). Only those sample members who worked during the follow-up period were included in these ANCOVA procedures. To correct for possible selection bias owing to the exclusion of nonworkers, inverse Mill's ratio terms were added, based on probit regressions using all sample members (see Heckman, 1979). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

working controls ($4.84) exceeded that for experimentals ($4.72). For other women, the second panel in Table 5.8, hourly wages for working experimentals were below those of working controls in the first two years of follow-up, then moved well ahead in the third and fourth years. For men, the bottom panel of the table, working experimentals earned slightly higher hourly wages than working controls in the first year, and earned nearly equal wages in succeeding years.

V. Impacts for Other Selected Subgroups

In addition to performing the split-sample analysis of earnings impacts presented above for the key subgroups, it is useful to estimate earnings impacts for subgroups controlling for differences in baseline characteristics other than that used to define the subgroups.[15] Such an analysis helps to determine whether the impact for the subgroup continues to be present even after differences in other characteristics are controlled for, or whether these other characteristics may be correlated to the subgroup's defining characteristic and have influenced the impact. This type of analysis also allows for a simpler determination of statistical significance for differences between subgroup impacts.

Using this type of analysis, Tables 5.9 and 5.10 present both subgroup and (where applicable) between-subgroup impacts on earnings for the entire four-year follow-up period and for the final follow-up year, respectively. As in earlier tables of this type, in Chapter 4, impacts are presented for the subgroups listed in the left-hand column of the table. In the middle of the table, earnings for experimentals and controls are shown, and the column labeled "subgroup impact" presents the experimental-control difference in earnings for each subgroup. For example, in the first panel of Table 5.9, the impact for women was $654 and for men, -$298. Neither subgroup impact was statistically significant, as shown by the lack of stars and the high p-value. The "subgroup impact difference" column shows the difference between the individual subgroup impacts in cases where there are only two subgroups in the category. In the case of women and men, the difference was $952, which was not statistically significant, again indicated by the lack of stars and the high p-value. In the second panel (ethnicity), no individual subgroup impact was significant, though whites had a large positive impact and the "other" subgroup had a large negative impact. The differences

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[15] This approach was discussed in Chapter 2 and used in Chapter 4 to examine subgroup differences in educational attainment.

TABLE 5.9

IMPACTS ON EARNINGS THROUGH MONTH 48, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

		Total Earnings, Months 1-48					
Characteristic and Subgroup	Sample Size	Experimentals ($)	Controls ($)	Subgroup Impact ($)	p[a]	Subgroup Impact Difference[b] ($)	p[a]
Gender						952	0.493
Women	1,041	12,435	11,781	654	0.488	---	---
Men	900	22,290	22,588	-298	0.770	---	---
Ethnicity						---	0.522
White, non-Hispanic	172	23,309	21,243	2,066	0.373	---	---
Black, non-Hispanic	860	14,410	14,250	160	0.878	---	---
Hispanic	847	18,434	18,182	252	0.810	---	---
Other	62	15,922	20,663	-4,741	0.225	---	---
Ethnicity, by gender						---	0.614
Women							
White, non-Hispanic	97	17,514	16,798	716	0.817	---	---
Black, non-Hispanic	467	10,332	10,487	-155	0.912	---	---
Hispanic	451	13,528	12,107	1,421	0.322	---	---
Other	26	13,404	13,595	-191	0.975	---	---
Men							
White, non-Hispanic	75	30,483	26,364	4,119	0.250	---	---
Black, non-Hispanic	393	18,930	18,577	353	0.819	---	---
Hispanic	396	24,183	25,358	-1,175	0.442	---	---
Other	36	19,560	27,625	-8,065	0.120	---	---
Parental status						---	0.782
Women living with own child(ren)							
No	533	12,596	11,666	930	0.481	---	---
Yes	508	11,531	11,165	366	0.787	---	---
Men who have own child(ren)							
No	785	22,470	22,488	-18	0.987	---	---
Yes	115	24,436	26,671	-2,235	0.436	---	---

(continued)

TABLE 5.9 (continued)

		Total Earnings, Months 1-48					
Characteristic and Subgroup	Sample Size	Experimentals ($)	Controls ($)	Subgroup Impact ($)	p[a]	Subgroup Impact Difference[b] ($)	p[a]
Employed within past year						-833	0.548
No	914	14,267	14,493	-226	0.822	---	---
Yes	1,027	19,453	18,846	607	0.523	---	---
Prior employment, by gender						---	0.629
Women employed within past year							
No	583	10,387	9,652	735	0.560	---	---
Yes	458	14,163	13,554	609	0.670	---	---
Men employed within past year							
No	331	18,271	20,164	-1,893	0.261	---	---
Yes	569	25,436	24,729	707	0.580	---	---
Left school in grade 11 or 12						-689	0.624
No	1,140	15,568	15,638	-70	0.939	---	---
Yes	801	19,054	18,435	619	0.566	---	---
Received occupational training within past year						-53	0.977
No	1,615	17,051	16,852	199	0.793	---	---
Yes	326	16,790	16,538	252	0.881	---	---
Age						770	0.624
16-19	1,425	17,381	16,961	420	0.604	---	---
20 or 21	516	15,990	16,340	-350	0.794	---	---
Age, by gender						---	0.725
Women							
16-19	763	13,071	11,917	1,154	0.300	---	---
20 or 21	278	10,401	11,153	-752	0.679	---	---
Men							
16-19	662	22,347	22,786	-439	0.712	---	---
20 or 21	238	22,362	22,310	52	0.979	---	---

(continued)

TABLE 5.9 (continued)

Characteristic and Subgroup	Sample Size	Total Earnings, Months 1-48 Experimentals ($)	Total Earnings, Months 1-48 Controls ($)	Subgroup Impact ($)	p[a]	Subgroup Impact Difference[b] ($)	p[a]
Marital status						3,905*	0.099
Ever married	184	20,721	16,970	3,751*	0.096	---	---
Never married	1,757	16,615	16,769	-154	0.832	---	---
Living in own household or with boy/girlfriend						89	0.960
No	1,575	16,615	16,367	248	0.746	---	---
Yes	366	18,752	18,593	159	0.920	---	---
Own AFDC case or receiving General Assistance						-79	0.960
No	1,418	17,544	17,351	193	0.811	---	---
Yes	523	15,564	15,292	272	0.838	---	---
Own AFDC case						24	0.988
No	1,522	17,401	17,182	219	0.779	---	---
Yes	419	15,589	15,394	195	0.896	---	---
Receiving Food Stamps						480	0.737
No	1,214	16,878	16,484	394	0.652	---	---
Yes	727	17,227	17,313	-86	0.939	---	---
Arrested since age 16						-991	0.611
No	1,649	17,089	17,024	65	0.931	---	---
Yes	292	16,556	15,500	1,056	0.557	---	---
Arrested since age 16, by gender						---	0.639
Women							
No	986	12,537	11,800	737	0.447	---	---
Yes	55	12,566	12,998	-432	0.917	---	---
Men							
No	663	22,262	23,183	-921	0.437	---	---
Yes	237	21,897	20,344	1,553	0.436	---	---

(continued)

TABLE 5.9 (continued)

Characteristic and Subgroup	Sample Size	Total Earnings, Months 1-48 Experimentals ($)	Total Earnings, Months 1-48 Controls ($)	Subgroup Impact ($)	p[a]	Subgroup Impact Difference[b] ($)	p[a]
Lived with both parents at age 14						-37	0.980
No	1,264	16,536	16,335	201	0.814	---	---
Yes	677	17,896	17,658	238	0.839	---	---
Reason for leaving regular high school						---	0.455
School-related	797	16,170	14,920	1,250	0.208	---	---
Job-related	161	22,022	22,840	-818	0.706	---	---
Other	699	16,877	17,249	-372	0.724	---	---

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.

TABLE 5.10

IMPACTS ON YEAR-FOUR EARNINGS, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

		Total Earnings, Months 37-48					
Characteristic and Subgroup	Sample Size	Experimentals ($)	Controls ($)	Subgroup Impact ($)	p[a]	Subgroup Impact Difference[b] ($)	p[a]
Gender						-124	0.819
Women	1,041	4,081	3,728	353	0.333	---	---
Men	900	7,342	6,865	477	0.223	---	---
Ethnicity						---	0.479
White, non-Hispanic	172	6,644	6,372	272	0.762	---	---
Black, non-Hispanic	860	4,522	4,523	-1	0.998	---	---
Hispanic	847	6,400	5,518	882**	0.030	---	---
Other	62	6,393	6,361	32	0.983	---	---
Ethnicity, by gender						---	0.766
Women							
White, non-Hispanic	97	4,730	5,327	-597	0.617	---	---
Black, non-Hispanic	467	3,265	3,387	-122	0.823	---	---
Hispanic	451	4,716	3,718	998*	0.073	---	---
Other	26	5,382	4,712	670	0.775	---	---
Men							
White, non-Hispanic	75	9,071	7,618	1,453	0.295	---	---
Black, non-Hispanic	393	5,910	5,822	88	0.882	---	---
Hispanic	396	8,373	7,656	717	0.226	---	---
Other	36	7,662	8,158	-496	0.805	---	---
Parental status						---	0.832
Women living with own child(ren)							
No	533	4,105	3,575	530	0.299	---	---
Yes	508	3,928	3,759	169	0.747	---	---
Men who have own child(ren)							
No	785	7,343	6,752	591	0.161	---	---
Yes	115	7,947	8,260	-313	0.778	---	---

(continued)

TABLE 5.10 (continued)

		Total Earnings, Months 37-48					
Characteristic and Subgroup	Sample Size	Experimentals ($)	Controls ($)	Subgroup Impact ($)	p[a]	Subgroup Impact Difference[b] ($)	p[a]
Employed within past year						-660	0.219
No	914	4,589	4,527	62	0.873	---	---
Yes	1,027	6,487	5,765	722**	0.050	---	---
Prior employment, by gender						---	0.673
Women employed within past year							
No	583	3,232	3,123	109	0.823	---	---
Yes	458	4,854	4,172	682	0.217	---	---
Men employed within past year							
No	331	6,087	6,097	-10	0.987	---	---
Yes	569	8,338	7,559	779	0.116	---	---
Left school in grade 11 or 12						329	0.546
No	1,140	5,250	4,704	546	0.118	---	---
Yes	801	6,084	5,867	217	0.602	---	---
Received occupational training within past year						163	0.819
No	1,615	5,668	5,239	429	0.143	---	---
Yes	326	5,187	4,921	266	0.684	---	---
Age						1,010*	0.097
16-19	1,425	5,856	5,176	680**	0.030	---	---
20 or 21	516	4,868	5,198	-330	0.526	---	---
Age, by gender						---	0.408
Women							
16-19	763	4,350	3,758	592	0.166	---	---
20 or 21	278	3,291	3,587	-296	0.674	---	---
Men							
16-19	662	7,604	6,821	783*	0.089	---	---
20 or 21	238	6,686	7,073	-387	0.616	---	---

(continued)

TABLE 5.10 (continued)

		Total Earnings, Months 37-48					
Characteristic and Subgroup	Sample Size	Experimentals ($)	Controls ($)	Subgroup Impact ($)	p[a]	Subgroup Impact Difference[b] ($)	p[a]
Marital status						581	0.526
Ever married	184	6,141	5,204	937	0.282	---	---
Never married	1,757	5,534	5,178	356	0.205	---	---
Living in own household or with boy/girlfriend						-20	0.975
No	1,575	5,498	5,087	411	0.167	---	---
Yes	366	6,010	5,579	431	0.484	---	---
Own AFDC case or receiving General Assistance						234	0.698
No	1,418	5,900	5,427	473	0.131	---	---
Yes	523	4,751	4,513	238	0.644	---	---
Own AFDC case						194	0.766
No	1,522	5,777	5,324	453	0.134	---	---
Yes	419	4,918	4,659	259	0.654	---	---
Receiving Food Stamps						755	0.172
No	1,214	5,731	5,037	694**	0.040	---	---
Yes	727	5,357	5,418	-61	0.888	---	---
Arrested since age 16						-923	0.222
No	1,649	5,613	5,341	272	0.349	---	---
Yes	292	5,471	4,276	1,195*	0.086	---	---
Arrested since age 16, by gender						---	0.452
Women							
No	986	4,100	3,716	384	0.305	---	---
Yes	55	5,076	4,801	275	0.864	---	---
Men							
No	663	7,391	7,268	123	0.789	---	---
Yes	237	6,943	5,379	1,564**	0.043	---	---

(continued)

TABLE 5.10 (continued)

Characteristic and Subgroup	Sample Size	Total Earnings, Months 37-48		Subgroup Impact ($)	p[a]	Subgroup Impact Difference[b] ($)	p[a]
		Experimentals ($)	Controls ($)				
Lived with both parents at age 14						-533	0.342
No	1,264	5,391	5,166	225	0.498	---	---
Yes	677	5,979	5,222	757*	0.094	---	---
Reason for leaving regular high school						---	0.796
School-related	797	5,249	4,650	599	0.120	---	---
Job-related	161	7,162	6,520	642	0.447	---	---
Other	699	5,649	5,406	243	0.553	---	---

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.

among these subgroup impacts also were not significant, as shown by the right-most p-value for that panel (0.522).[16]

The subgroups based on parenting status by gender also show stronger impacts for women than for men, and for other women than for custodial mothers, although here again the differences in impacts were not statistically significant. Thus, the earlier observed differences in 48-month earnings impacts for men, custodial mothers, and other women did not disappear (although they were not statistically significant) when other measured characteristics of each subgroup were controlled for in the calculations. The observed male-female earnings differences in impacts do not appear to have resulted from other measured non-gender differences in the sample members' pre-random assignment characteristics (such as differences in education or work experience).

Among the other subgroup impacts on earnings presented in Tables 5.9 and 5.10, all but two of the subgroup impact differences (to the right of the rule) were insignificant in a statistical sense. The exceptions were the final-year earnings impact (Table 5.10) for the two subgroups defined by age alone, where the younger subgroup had a larger impact. Four-year earnings impact differences between subgroups seem sizable in absolute terms (a more than $800 difference between the highest and lowest subgroup impacts within a category) for ethnicity, ethnicity by gender, parenting status, prior employment by gender, age by gender, marital status, arrest history, arrest history by gender, and reason for leaving regular high school.

Reviewing the findings presented in this section, there is no clear pattern among the differences in four-year earnings impacts for subgroups. In some cases, subgroups with sample members who had greater barriers to employment (represented by lower control group earnings) had greater impacts. Women had larger impacts than men, and those arrested between age 16 and random assignment had larger impacts than those who had not been arrested. For other subgroups, however, the opposite pattern emerged. Impacts were higher, for example, for those with recent work experience. For some subgroups, most notably those based on ethnicity, no clear pattern emerged as to how impacts were related to employability. For example, for the entire four-year period, white, non-Hispanic men and Hispanic women had the highest earnings impacts, and men in the "other" subgroup (primarily Asians and Native Americans) had large negative impacts.

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[16] As mentioned in Chapter 4, no single subgroup impact difference can be calculated for characteristics such as ethnicity that have more than two subgroups; consequently, for those characteristics the subgroup impact difference column has no number.

These mixed findings differ from the results of several studies of employment programs for welfare recipients that did find patterns among subgroup impacts.[17] In these programs, impacts tended to be small or nonexistent for the most and least job-ready, and positive for those who fell between these two groups. The subgroups used in the JOBSTART analysis were much more narrowly defined, which may have contributed to the absence of a pattern in the impacts.

VI. The Influence of Program Characteristics on Earnings Impacts

Earlier sections of this chapter provided estimates of JOBSTART's impacts on earnings for the full sample and key demographic subgroups. This final section addresses another important issue: Were these impacts affected by program characteristics? The most direct way to explore this issue within the context of the JOBSTART Demonstration is to examine differences in individual *site* impacts, since there were some programmatic differences among the 13 sites and the programs operated in a variety of settings.

This analysis of earnings impacts across sites, of necessity, is less certain than that for the full sample and subgroups for two major reasons. First, the JOBSTART sites vary on many dimensions, so it is difficult to isolate the influence of any single factor on site-level impacts.[18] The various ways in which the sites differed are briefly summarized below. Second, the impacts on earnings at the individual site level (even when noticeably different from zero) were usually not statistically significant. This is the case because the sample size in each site was relatively small; a site's program did not affect the behavior of all youths in the same way and controls' earnings also varied (in technical terms, these last two points suggest a large variance in outcomes within each site); and many of the reported earnings impacts were not large. Furthermore, the *differences* in the individual site impacts on employment and earnings were also not statistically significant, suggesting again that lessons on program structure can only be tentative.[19]

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[17] Gueron and Pauly, 1991.

[18] To answer the question of how program characteristics affected impacts with the same methodological rigor as that of the previous analysis of earnings impacts, youths *in individual sites* would have had to be randomly assigned to one of the several types of programs of interest (for example, one emphasizing education or one emphasizing training), and individual sites would have had to offer more than one type of program. This kind of study – known as a "differential impact" research design – is needed to separate the influence of program type from other site variations such as local labor market conditions and differences in the kinds of youths drawn to the sites.

[19] In this context, statistical significance indicates whether the extent of variation in impacts *across* sites was so systematic that it was unlikely to have arisen by chance.

Nevertheless, the JOBSTART Demonstration did provide an opportunity to learn more about the link between program characteristics and impacts. For example, if site categories based on specific characteristics such as program structure or sponsoring organization had shown clear differences in impacts, certain lessons might have been drawn (including the *lack* of influence of other factors).

As the findings in this section show, however, no clear pattern emerged: that is, no one site category did appreciably better than the others in terms of four-year impacts. Instead, both positive and negative impacts appeared within each category, suggesting that JOBSTART can produce positive or negative impacts in a variety of settings and with a variety of program structures.

Much of the previous analysis in this report divided sites into three groups based on program structure – concurrent, sequential/in-house, and sequential/brokered – because these categories helped explain differences in program implementation and participation among the sites.[20] This chapter preserves these site groupings, since the implementation research led to a hypothesis that these differences in program structure could influence impacts. But it is important to keep in mind that these categories are not a neat division of the sites into homogeneous groups.[21] Thus, disentangling the independent effect of any single program feature on impacts across sites can be very difficult.

Despite these difficulties in analysis and interpretation, this chapter does try to draw some conclusions about the link between program features and impacts. It attempts this type of analysis because of the strong interest in identifying more effective ways to structure education and training programs.[22] The framework for analyzing site-level impacts is presented below, followed by a discussion of site-level impacts for the full 48-month sample.

A. A Framework for Analyzing Site Variation in Impacts

The differences in program structure among the sites (concurrent, sequential/brokered, and sequential/in-house) were among the most obvious possible influences on site impacts. However, these program factors are entangled with many other sources of variation across sites, including the target group recruited at various sites; sites' greater interest in education or training; the extent of screening by site operators; the point at which random assignment took place for the evaluation; the availability of alternative local services for members of the control group; and environmental factors such as the wage structure and tightness of the labor market.

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[20] For example, the sequential/brokered sites all had difficulty getting young people to make the transition to training, and the concurrent sites all tended to emphasize training (as opposed to education) more than the other sites.

[21] One key difference within the concurrent category was discussed earlier in the report: Two sites (CET/San Jose and Chicago Commons) integrated education into the training curriculum to a much greater extent than the other concurrent sites. Other characteristics that varied within categories included the extent to which applicants were initially screened, the quality of sites' implementation of the four JOBSTART components, and local labor market conditions.

[22] This interest was intensified by the recent publication of the impact report on the Minority Female Single Parent (MFSP) Demonstration, sponsored by The Rockefeller Foundation. See Burghardt et al., 1992.

1. **Program structure.** As detailed in Chapter 3, dimensions of program structure that are of special interest include concurrent versus sequential education and training classes; the extent of integration of education and training; brokering of services among multiple vendors versus in-house provision of all services; months of program activities offered and delivered; the relative emphasis on education as opposed to occupational skills training; the strength of job placement efforts; and the intensity of support services. Table 5.11 (which is the same as Table 3.11) highlights key aspects of the sites' program structure. Sites are grouped under the headings *concurrent*, *sequential/in-house*, and *sequential/brokered*.

"Average total hours" is one measure of the intensity of program services in a site and "average length of stay" is an important dimension of site variation because it can influence how long participants lose initial earnings because they are active in the program.[23] "Average hours per month" is a measure of the extent to which education, training, and other activities were concentrated or spread out over time. In general, the shorter the length of stay, the higher were the hours per month. CET/San Jose and Chicago Commons, the two sites with the shortest length of stay, had high average hours per month because they squeezed just about as much program activity as the average site offered into shorter periods of time.[24]

The "average hours of education" column shows much variation, but there was a tendency for the sequential – and especially sequential/brokered – sites to emphasize education more than the

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[23] In sites where experimentals participated for shorter periods of time, there was more post-program time during which experimental employment outcomes could have overtaken control outcomes during the four-year follow-up period. CET/San Jose and Chicago Commons were notable on this score, with average lengths of stay less than two-thirds of the full-sample average, even though total hours were higher than average at Chicago Commons and 84 percent of the average at CET/San Jose, suggesting that shorter lengths of stay are not necessarily correlated to lower intensity of program services.

[24] CET/San Jose and Chicago Commons had two things in common that help explain their relatively short length of stay. First, the programs were concurrent, offering education and training at the same time rather than one after the other. Second, there was less emphasis on education leading to a GED than on skills training in these integrated or partly integrated programs. Thus, youths in these programs typically stayed fewer months than those in sequential programs and in other concurrent programs that emphasized education more. For these reasons, one might hypothesize that earnings losses would have been confined to a shorter period in these two sites than in sites that provided the same level of services over a longer period of time.

TABLE 5.11

SUMMARY OF JOBSTART IMPLEMENTATION, BY SITE

Site	Average Total Hours	Average Length of Stay (Months)	Average Hours per Month	Average Hours in Education	Average Hours in Training	Level of Initial Screening	Rating of Job Placement	Rating of Support Services	Overall Rating of Implementation	JOBSTART Operating Costs per Experimental ($)[a]
Concurrent	387	6.1	63	101	276	---	---	---	---	---
Atlanta Job Corps	334	5.8	59	102	177	High	Low	High	Low	4,100[b]
CET/San Jose[c]	335	4.1	82	26[d]	309	Low	High	Medium	High	2,000
Chicago Commons[c]	422	4.3	98	69[d]	353	High	High	Medium	High	6,400
Connelley (Pittsburgh)	538	9.5	57	99	439	Medium	Medium	High	High	5,200
East LA Skills Center	370	5.7	65	76	294	Medium	Low	Medium	Medium	4,900
EGOS (Denver)	270	7.1	38	128	142	Low	Low	Medium	Low	2,000[b]
Phoenix Job Corps	420	6.3	67	161	202	High	High	High	High	4,700[b]
SER/Corpus Christi	400	5.0	80	118	282	Medium	Low	High	Medium	2,100
Sequential/in-house	518	7.0	75	146	276	---	---	---	---	---
El Centro (Dallas)	408	6.0	68	147	179	Medium	Medium	Medium	High	5,100
LA Job Corps	607	7.8	79	146	355	High	Medium	High	High	5,700[b]
Sequential/brokered	307	7.7	39	176	71	---	---	---	---	---
Allentown (Buffalo)	427	10.7	38	238	105	Medium	Low	High	Medium	5,900
BSA (NYC)	282	5.6	49	149	63	Low	Low	Medium	Low	7,500
CREC (Hartford)	171	6.3	29	124	35	Low	Low	Medium	Low	5,200
All sites	398	6.6	60	125	238	---	---	---	---	---

SOURCES: MDRC calculations from JOBSTART enrollment form and MIS data (participation figures); MDRC operations staff (implementation ratings); Appendix C (costs).

NOTES: Calculations for this table used data for all 988 experimentals for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

[a] These cost estimates are rounded to the nearest $100. All costs are in 1986 dollars.

[b] The estimates do not include the cost of providing on-site medical and dental services. The value of these services per JOBSTART experimental was approximately $400 in the Atlanta site, $400 in Phoenix, $800 in Los Angeles, and $24 in Denver.

[c] In this site, education and training were more integrated than in other sites, and staff strongly emphasized training over passing the GED examination.

[d] In this site, some education hours are included in the training component hours.

concurrent sites.[25] The "average hours in training" column shows even more site variation than the previous columns. As already indicated, the dramatic difference between the sequential/brokered sites and the others on this score probably reflected both the systemic and administrative problems the former encountered in linking education and training, and differences in the interests and expectations of recruits.[26] One surprise here is that the two sequential/in-house programs managed to deliver hours of training comparable to those in concurrent programs.[27] The rating of job placement is a subjective ranking of the strength of job placement assistance efforts in a site, and the rating of support services assesses the strength of the implementation of that component of JOBSTART.[28]

To sum up Table 5.11, CET/San Jose, Chicago Commons, Connelley in Pittsburgh, El Centro in Dallas, and the Los Angeles and Phoenix Job Corps programs seem to have implemented JOBSTART's four components more fully than did the other sites. Thus, these sites appear to have set the stage for stronger program impacts than those in the other sites. However, the relatively strong treatments, short lengths of stay, and resulting intensity (hours per month) at CET/San Jose and Chicago Commons could have given these two sites an edge over the others by lessening initial earnings losses during the period of program participation.

2. Differences in applicant characteristics. While all JOBSTART sites had a common target group for the demonstration (disadvantaged young dropouts reading below the eighth-grade level), there were variations in site recruiting emphases and in participant characteristics. These site variations arose because different types of youths applied and because intake practices were not uniform.

Some characteristics of youths varied in easily measured ways. For example, as reported in Chapter 2, sites varied greatly in the proportion of the sample that was made up of youths who had recent work experience or who were custodial mothers. Factors such as these were measured at random assignment and can, to a considerable extent, be adjusted for statistically in comparing program impacts across sites. One type of impact estimate reported later in this section does include such statistical adjustments in an effort to control for these measured differences in sample characteristics among sites and to move closer to isolating variation in impacts caused by program structure.[29]

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[25] Three of the eight concurrent sites averaged less than two-thirds of the average for all sites, and only one of them substantially exceeded the all-site average. Variation in the proportion of youths receiving any education was not a major factor in site variation in hours, except at CET/San Jose (which did not offer separate education classes), where the proportion varied in a narrow range around the full-sample average.

[26] Only one-quarter of experimentals at sequential/brokered sites got any occupational skills training at all.

[27] This achievement is notable because only half of the experimentals in sequential/in-house sites stayed in the program long enough to receive any training, while more than 80 percent of experimentals in concurrent sites received training.

[28] Costs, in the right-hand column, are discussed in more detail in Chapter 7.

Some sample characteristics are much harder to observe and therefore were not measured at entry into the sample; these cannot be included in statistical adjustments. One especially important factor is the youths' goals for participating in the program and their resulting interest in the various JOBSTART components. Although the youths' goals and interests were not observed directly in any quantifiable way, the reputation of the site in the community and its perceived service emphasis probably had a strong influence on who applied for the program.[30]

3. **Screening**. Some sites – such as CET/San Jose, EGOS in Denver, BSA in New York City, and CREC in Hartford – did not screen applicants much before they entered the sample, while others (such as Chicago Commons and the Job Corps sites) carried out extensive screening.[31] When a great deal of screening takes place, it is reasonable to assume that those randomly assigned (including those assigned to control group status) will have high levels of perseverance, motivation to get into a program, motivation to work, and other important characteristics hard to measure directly. Thus, all else being equal, in sites that screened heavily, control group outcomes should have constituted tougher benchmarks for experimentals to surpass

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[29] Although these impacts include linear statistical adjustments for sample characteristics at random assignment, not all relevant characteristics were measured; there might be differential errors of measurement of characteristics; true relationships between impacts and characteristics may be nonlinear; or impacts for sites with very unusual sample characteristics might be difficult to adjust for properly with any statistical model. Thus, adjusted site outcomes and impacts must be viewed with a special caution not applicable to estimated subgroup impacts.

[30] Those sites with a history of providing basic skills instruction and not occupational training (BSA in New York City, Allentown in Buffalo, and CREC in Hartford) naturally would seem to have attracted youths who were more interested in GED attainment than in immediate acquisition of marketable occupational skills, while those sites emphasizing occupational skills training (CET/San Jose and Chicago Commons) seem to have attracted youths who were more interested in job skills and immediate employment than in education. Available information does indirectly support this generalization. Table 5.11 shows that JOBSTART hours of education tended to be high and that hours of occupational skills training were lowest in sequential/brokered sites, although the latter clearly resulted partly from the administrative problems those sites encountered in linking education and training. Taken together, these findings are consistent with the idea that recruits in the sequential/brokered sites were less interested in occupational training per se than in education leading to a GED. In other words, these sites may have delivered fewer hours of skills training partly because their recruits demanded fewer hours.

[31] Chapter 3 pointed out the severe recruitment pressures at CET/San Jose during intake for JOBSTART. CET/San Jose may have been able to adopt this "no screening" policy because of the characteristics of disadvantaged youths in its service area. Even with open admissions, a higher percentage of the CET/San Jose youths had recent work experience than did the full sample. This probably occurred because CET/San Jose drew largely Hispanic youths, a group that in the western states typically has a higher labor force participation rate than other minority school dropouts.

than in sites that carried out minimal screening. However, minimal screening may yield groups of experimentals who are harder to keep in the program and harder to place in jobs at completion.

4. **Point of random assignment.** In addition to the normal steps and resulting screening in recruitment and intake, the research design introduced random assignment, which could occur in different sites at slightly different points relative to initial contact with a potential sample member and actual start-up of services. As mentioned in Chapter 2, putting random assignment early in the program flow (as at CET/San Jose) tended to include in the sample individuals who might drop out during the later steps of intake, while putting random assignment relatively late (as at Allentown in Buffalo) meant that only those making it to that stage of intake were in the research sample. Late random assignment tended to raise the proportion of the experimental group participating in JOBSTART, because it meant that those randomly assigned were interested enough in the program to persevere through the steps of intake and because it reduced the waiting period between random assignment and the start of services. Late random assignment also tended to raise the proportion of controls who were served in alternative programs, because those randomly assigned to the control group were also quite motivated to receive services.[32]

5. **Services received by controls.** Since the impacts reported here are the effects of JOBSTART compared to the alternative services in which the controls participated (rather than those of JOBSTART versus no services), variation in the controls' benchmark levels of services will influence impacts. As discussed in Chapter 3, in most sites more than 90 percent of experimentals were active in education or training at some point in the four-year follow-up period. There was one notable exception: At CET/San Jose, a lower than average percentage of experimentals (approximately 75 percent) participated during the four years because of program funding problems early in random assignment. In contrast to the consistent and high level of participation among experimentals, the level of control services varied greatly by site. Over the four years, the percentage of controls active in education or training ranged from a high of 78 percent at the Atlanta Job Corps to a low of 40 percent at El Centro in Dallas. Consequently, the service receipt differences between experimentals and controls also varied greatly by site, from a high of 60.5 percentage points at El Centro to a low of 19.2 percentage points at the Atlanta Job Corps.[33]

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[32] The correlation coefficient of sites' percentage of experimentals and controls participating in education and training is +.37. The point of random assignment thus had implications for the proportion of experimentals and controls receiving services, but both groups were affected similarly, so there was no consistent effect on the *difference* in the proportion of experimentals and controls receiving some type of education or training.

[33] See Cave and Doolittle, 1991, for the details of these differences in service receipt across sites.

Because of the low percentage of experimentals active in JOBSTART at CET/San Jose, the site had a service receipt differential of 27 percentage points.

6. Wage structure and labor market tightness. In some communities, jobs tend to be plentiful but low-paying; in others, they tend to be scarcer but higher-paying; and in still others, they are both scarce and low-paying. This could be the result of long-term differences in the industrial base of the communities or of how they are affected by business cycles. These labor market conditions could affect both the control group's level of employment and earnings and the availability of jobs that reward the experimental group's increased educational attainment and occupational training.

B. Two Alternative Types of Site Earnings Impact Estimates

As just discussed, sites differed in many ways, making it very difficult to isolate the influence of program features on impacts. The most straightforward method of estimating impacts is to calculate separate ("split sample") experimental-control comparisons for each site, in effect creating 13 separate samples (or data files) and calculating impacts for each one. In doing this type of impact analysis for sites, the only adjustment made is to take account of differences in the observed pre-random assignment characteristics of the site's experimental and control groups, which may occur when sample sizes are relatively small.

It is possible to move beyond this type of analysis because one source of site variation – the observed characteristics of the youths in the sample – can be adjusted for statistically by relying on the fact that youths with particular characteristics are rarely concentrated in just one site. This type of adjustment takes account of the program impacts for each type of youth in the full 13-site sample (for example, the impacts for blacks, those with prior work experience, or those receiving public assistance) and adjusts individual site impacts to reflect the fact that some sites served many youths who (in the full sample) tended to have lower than average impacts, while other sites served more than the usual percentage of youths who had higher than average impacts.[34]

The two analytical approaches answer different questions and each has strengths and weaknesses. The split-sample approach is most valuable as a description of what actually occurred in each site: that is, it presents the best estimates for the impacts of the site's program as it actually operated for the people recruited and randomly assigned to the experimental group.[35] Its chief drawback is that the differences in outcomes for experimentals and controls within each site are not fully comparable across sites because the characteristics of the two groups differ from site to site. If, for example, youths who were employed in the year prior to random assignment tended to have lower employment and earnings impacts, sites with a sample made up of youths with higher than average prior-year employment will have lower impacts than if they had served more disadvantaged recruits. Furthermore, the individual sites may have relatively small sample sizes, limiting the precision of impact estimates.

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[34] The statistical adjustment assumes a linear relationship between each characteristic and outcomes, and is the same assumption underlying the method in Tables 4.4, 4.7, 5.9, and 5.10, which present impacts for designated subgroups controlling for differences between the subgroups other than the characteristic used to define them. These tables used linear adjustments of outcomes, with differences in youths' characteristics entered as covariates. See Ostle, 1975.

The second approach, adjusting for measured site differences in pre-random assignment characteristics, is most valuable in trying to isolate the effects of location per se. Sites with populations different from the overall average are not penalized or rewarded relative to other sites, as they are in the split-sample approach. For example, the estimated impacts for a site with a higher than average proportion of youths with prior employment would be larger using this adjustment than using the split-sample approach. However, in order to make the adjustment for site differences in the mix of youths' pre-random assignment characteristics, it is necessary to assume that sites could have served – and achieved "average" impacts for – populations they actually did not serve. For example, sites set up to serve a particular type of participant (for example, young men) might not be able to serve other young people. Thus, they might not attain the impact observed in the full sample for these other groups.[36]

The outline of the site impact story is the same using both types of approaches, although the adjustment does make a noticeable difference in the impacts of a few sites that served a group of young people that differed markedly from the sample as a whole and/or had smaller than average sample sizes. Table 5.12 presents the split-sample impacts, that is, those not adjusted for differences

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[35] This was the approach taken, for example, in the recent report on the Minority Female Single Parent (MFSP) Demonstration; the findings for each of the four sites were calculated separately. See Burghardt et al., 1992.

[36] The adjusted impact approach uses the entire sample, lessening the problems of sample size in the analysis. However, it introduces a further complication: The adjustments implicitly assume that youths' characteristics affect outcomes in the same way in each site. More technically, the assumption is that the coefficients estimated for the regression used in adjusting for differences in characteristics (both between experimentals and controls and across sites) are the same across all sites. The split-sample approach estimates separate regressions for each site to adjust for differences in sample characteristics between experimentals and controls. Therefore, the shift from split-sample impacts to those adjusted for site differences in pre-random assignment characteristics involves two changes: changing to the standard full sample regression coefficients and adjusting for differences across sites in sample characteristics. Consequently, it is often not possible to give a simple, intuitive explanation of why the shift from split-sample to adjusted impacts caused the observed change in impacts.

TABLE 5.12

IMPACTS ON EARNINGS, BY SITE
(NOT ADJUSTED FOR SITE DIFFERENCES IN SAMPLE CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT)

Site	Sample Size	Total Earnings, Months 25-48 Experimentals ($)	Controls ($)	Difference ($)	Total Earnings, Months 1-48 Experimentals ($)	Controls ($)	Difference ($)
Concurrent							
Atlanta Job Corps	69	10,492	8,241	2,251	15,123	16,498	-1,375
CET/San Jose	167	20,808	14,721	6,087***	32,959	26,244	6,715**
Chicago Commons	75	9,015	12,471	-3,456	16,346	19,172	-2,826
Connelley (Pittsburgh)	184	6,188	5,148	1,040	9,047	8,125	922
East LA Skills Center	106	15,857	14,542	1,315	23,705	23,586	119
EGOS (Denver)	198	10,415	11,441	-1,026	15,381	17,828	-2,447
Phoenix Job Corps	134	6,835	9,685	-2,850	11,393	17,729	-6,336**
SER/Corpus Christi	247	11,085	10,401	684	16,075	17,244	-1,169
Sequential/in-house							
El Centro (Dallas)	179	11,018	9,989	1,029	16,568	14,030	2,538
LA Job Corps	231	11,710	11,683	27	17,758	18,869	-1,111
Sequential/brokered							
Allentown (Buffalo)	135	5,556	5,081	475	9,338	8,229	1,109
BSA (NYC)	117	9,771	10,782	-1,011	15,465	21,642	-6,177
CREC (Hartford)	99	8,893	9,897	-1,004	17,302	17,268	34

SOURCES: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for 19 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

across sites among the youths served. Table 5.13 presents impacts that have been adjusted for these differences. Both tables present impacts per experimental and include two impacts: The left panel presents experimental-control differences in earnings during the third and fourth years of follow-up and the right panel presents differences in earnings over the entire four years of follow-up.

As anticipated, few of the individual site impacts were statistically significant, so any lessons would come from patterns of impacts across sites. In both tables, in each site grouping, there are one or more sites with a positive impact and one or more with a negative or very small positive impact for each of the two time periods. As the tables show, there was variation in impacts *within* each group of sites as well as *between* groups of sites. Impacts on third- and fourth-year earnings showed great apparent variation across sites (though the differences were not statistically significant) and considerable apparent variation within each of the three groups of sites. Because of the diverse earnings of youths within the individual sites and the small samples (owing to the fine level of disaggregation), only one of the 13 individual site impacts for years three and four was statistically significant in each table, and for the entire four-year follow-up period, Table 5.12 shows only two significant impacts and Table 5.13 only one. It is notable that there were positive and negative earnings impact estimates in each of the three groups of sites in both tables.

The most striking finding in these tables is the strong impacts for CET/San Jose. In both tables, and for both the last two years and the entire four-year follow-up period, earnings impacts were large, positive, and statistically significant. These occurred despite CET/San Jose's lower than average experimental-control service receipt differential, discussed above. Further, because of experimentals' lower than average participation rate at CET/San Jose, its impacts per JOBSTART participant would be substantially higher than at other sites.[37] These findings, coupled with CET/San Jose's large positive impacts in the Minority Female Single Parent study (see Burghardt et al., 1992), are mounting evidence of the effectiveness of this program.

That said, it is very difficult to identify what features of the CET/San Jose approach led to its strong impacts. Site characteristics include a relatively concentrated dose of JOBSTART (i.e., a short average length of stay but near-average total hours), strong job placement efforts, little up-front screening of applicants, close ties to the employer community, integration of education and training, a long-standing good reputation in the local communities, a clear organizational mission,

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[37] Impacts per JOBSTART participant can be calculated by dividing impacts per experimental by their participation rate.

TABLE 5.13

IMPACTS ON EARNINGS, BY SITE (ADJUSTED FOR SITE DIFFERENCES IN SAMPLE CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT)

		Total Earnings, Months 25-48			Total Earnings, Months 1-48		
Site	Sample Size	Experimentals ($)	Controls ($)	Difference ($)	Experimentals ($)	Controls ($)	Difference ($)
Concurrent							
Atlanta Job Corps	69	12,205	10,112	2,093	18,447	19,234	-787
CET/San Jose	167	18,909	12,362	6,547***	29,600	22,252	7,342***
Chicago Commons	75	10,309	11,726	-1,417	18,125	18,296	-171
Connelley (Pittsburgh)	184	7,470	6,685	785	10,935	10,375	560
East LA Skills Center	106	14,501	13,158	1,343	22,002	21,355	647
EGOS (Denver)	198	11,091	10,690	401	16,268	16,920	-652
Phoenix Job Corps	134	6,873	8,198	-1,325	11,396	14,509	-3,113
SER/Corpus Christi	247	8,477	7,992	485	11,705	13,327	-1,622
Sequential/in-house							
El Centro (Dallas)	179	11,393	11,057	336	17,111	15,506	1,605
LA Job Corps	231	12,636	12,757	-121	19,570	20,789	-1,219
Sequential/brokered							
Allentown (Buffalo)	135	7,481	6,577	904	12,589	11,174	1,415
BSA (NYC)	117	11,923	10,499	1,424	19,682	20,825	-1,143
CREC (Hartford)	99	9,754	11,124	-1,370	19,017	19,302	-285

SOURCES: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

and experienced and skilled staff. It also operated in an unusual labor market, as evidenced by the high control group earnings throughout the follow-up period.[38]

As an example of the difficulty of isolating the influence of individual program features, Chicago Commons was the site most similar to CET/San Jose (although it screened applicants more, the labor market in Chicago was very different from that in San Jose, and training tended to be concentrated in a smaller number of occupations). In the first two years of follow-up, earnings impacts adjusted for differences in sample characteristics were positive in the two sites, and impacts in the second year (a post-program period for most experimentals) were the largest of any sites. However, in the third and fourth years of follow-up, earnings impacts continued to be strongly positive in CET/San Jose while turning strongly negative for Chicago Commons.[39]

In addition to CET/San Jose, five other sites had positive earnings impacts for years three and four of follow-up in excess of $1,000 in Table 5.12 or 5.13, and their diversity illustrates again the difficulty of drawing conclusions. The Atlanta Job Corps, Connelley in Pittsburgh, and East LA Skills Center were all concurrent sites, with the latter two based in adult vocational schools. El Centro in Dallas, based in a community college, operated a sequential/in-house program and Basic Skills Academy in New York City was a community-based organization operating a sequential/brokered program with a heavy emphasis on education.

Despite strong policy interest in the influence of program structure on impacts, no simple story emerges from the data. JOBSTART was sometimes effective and sometimes ineffective in yielding labor market gains in brokered programs and in-house programs; it also showed varying success in both concurrent and sequential programs. This suggests that the JOBSTART program model can be implemented successfully in a variety of settings. The one clear conclusion from this analysis is the growing support for the effectiveness of the CET/San Jose program.

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[38] CET has received a grant from the U.S. Department of Labor to replicate its program in 10 sites. This effort includes four sites where CET will initially operate the local programs itself and six sites where CET staff are training other organizations.

[39] One hypothesis, which cannot be tested with available data, is that Chicago Commons' focus on specialized training for a few occupations in high demand in the local economy initially paid off for participants, but then the labor market changed and participants were vulnerable.

CHAPTER 6

IMPACTS ON NON-LABOR MARKET OUTCOMES

Chapters 4 and 5 presented estimates of JOBSTART's impacts on educational attainment, employment, and earnings for the full 48-month sample and key subgroups. If JOBSTART was successful, other aspects of the JOBSTART youths' lives may also have changed. The following questions about non-labor market outcomes are addressed in this chapter for the full sample and for subgroups among which there are notable differences in impacts, including subgroups defined by gender, parenting status, and – for some outcomes – criminal record prior to random assignment.[1]

- Did JOBSTART reduce receipt of public benefits?
- Did JOBSTART lead to changes in household composition, for example, by reducing the birth rate or increasing the marriage rate?
- Did JOBSTART reduce criminal arrest rates and drug use?
- Did JOBSTART lead to more "productive activity," defined as either working or further education and training?[2]

Among the outcomes analyzed in this chapter, impacts for the full sample were rarely large or statistically significant. When impacts were large or significant, they tended to be for the first 12 or 24 months of follow-up, essentially the in-program period and the following year.[3] Among the subgroups, women who were not custodial mothers at random assignment and men who had been arrested between age 16 and random assignment had more encouraging impacts on some outcomes. More specifically:

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[1] The samples for the impact analyses in this chapter vary slightly owing to missing data for some outcomes. In all cases, the sample includes at least 95 percent of the 48-month impact sample analyzed in the previous chapters.

[2] During the latter three years of follow-up, the employment rates of experimentals and controls were quite similar: Thus, the answer to this fourth question depends on whether there were differences in how experimentals and controls spent nonworking time.

[3] A pattern of encouraging in-program impacts that disappeared in the initial years of post-program follow-up was seen in several prior studies of youth programs, including evaluations of Project Redirection for young teenage mothers (Polit, Quint, and Riccio, 1988) and the Summer Training and Education Program (STEP) for at-risk high school students (Walker, 1992). However, in the case of Project Redirection, analysis of five-year follow-up data showed a reappearance of several positive non-labor market impacts. Similar long-term follow-up data for STEP were not available.

- **For women who were not custodial mothers**, there was a consistent pattern of substantial – and for some periods significant – impacts on AFDC receipt, and small, insignificant impacts on pregnancy and childbirth rates (that is, the rates were somewhat lower for experimentals than controls). The pattern of findings over the four-year period indicates that JOBSTART appears to have lowered the probability that this group of young women would receive AFDC benefits, in effect serving as a welfare prevention program.

- **For custodial mothers**, experimentals had significantly higher rates of pregnancy and childbirth than controls, and – related to these findings – a mixed pattern of impacts on receipt of public benefits. Because the increase in births occurred primarily among custodial mothers who had been married at some time prior to random assignment, there was no significant increase in AFDC receipt (the program principally serves single parents), but there were some increases in receipt of other public benefits.[4]

- **For all men** in the sample, JOBSTART had little impact on any of the outcomes studied in this chapter. In general, public benefits were a much less important source of income for men than women and there were small experimental-control differences in these outcomes. Further, the arrest rates for experimentals and controls over the four years were very similar.

- **For men with an arrest between age 16 and random assignment** – an especially disadvantaged subgroup – JOBSTART led to a significant decline in the use of drugs other than marijuana.

In determining the effect of JOBSTART on public assistance receipt, pregnancy and childbirth, drug use, and criminal arrests, this chapter presents a vivid picture of the lives of JOBSTART sample members, a group of disadvantaged young people.

I. Impacts on Receipt of Public Assistance and Other Public Benefits

Table 6.1 summarizes the impacts of JOBSTART on receipt of AFDC, Food Stamps, and General Assistance for the full sample. For each benefit, the table shows impacts on the percentage ever receiving the benefit and on benefit income received for the four-year follow-up period and for individual years.

In general, there were very small differences in the percentage of experimentals and controls in the full sample who ever received these benefits, during either the entire follow-up period or individual years. Over the full 48 months, virtually the same percentage of controls and

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[4] AFDC was available to two-parent families during the follow-up period in many states, but there were work history requirements for the "principal earner" that could make young parents ineligible. Food Stamps and some other forms of public assistance do not have such restrictions regarding eligibility.

TABLE 6.1

IMPACTS ON AFDC, FOOD STAMPS, AND GENERAL ASSISTANCE THROUGH MONTH 48

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Ever received AFDC (%)				
Years 1-4	35.4	35.1	0.3	0.862
Ever received AFDC (%)				
Year 1	20.5	19.3	1.2	0.373
Year 2	26.5	26.5	0.0	1.000
Year 3	23.8	24.5	-0.7	0.677
Year 4	26.5	27.4	-0.9	0.587
Total AFDC income ($)				
Years 1-4	3,636	3,562	74	0.750
AFDC income ($)				
Year 1	687	624	63	0.243
Year 2	840	816	24	0.703
Year 3	984	987	-3	0.972
Year 4	1,125	1,136	-11	0.897
Ever received Food Stamps (%)				
Years 1-4	58.2	60.1	-1.9	0.327
Ever received Food Stamps (%)				
Year 1	36.9	38.5	-1.6	0.381
Year 2	44.9	46.4	-1.4	0.446
Year 3	37.4	36.5	0.9	0.641
Year 4	43.4	43.1	0.3	0.879
Total Food Stamp income ($)				
Years 1-4	2,508	2,532	-24	0.839
Food Stamp income ($)				
Year 1	523	568	-45	0.207
Year 2	537	579	-42	0.228
Year 3	658	627	31	0.449
Year 4	790	759	31	0.493
Sample size	962	916		

(continued)

TABLE 6.1 (continued)

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Ever received General Assistance (%)				
Years 1-4	11.2	10.8	0.4	0.743
Ever received General Assistance (%)				
Year 1	6.5	5.2	1.2	0.201
Year 2	6.4	6.0	0.4	0.717
Year 3	3.9	4.0	-0.1	0.949
Year 4	5.0	4.6	0.4	0.677
Total General Assistance income ($)				
Years 1-4	389	360	29	0.653
General Assistance income ($)				
Year 1	131	107	24	0.308
Year 2	61	54	7	0.644
Year 3	89	95	-6	0.809
Year 4	107	104	3	0.910
Sample size	962	916		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcomes, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

experimentals ever received AFDC or General Assistance, while slightly more controls ever received Food Stamps, but this small difference was not statistically significant. Impacts for individual years were similarly small, in no case exceeding two percentage points.

Impacts on income received from the various benefits were also small and insignificant. In calculating these impacts, sample members who did not receive a benefit were counted as receiving zero income from that benefit, and were included when averaging the total benefit income received by experimentals and controls. For example, calculations of total AFDC income over the four-year follow-up period – including those who received and did not receive AFDC – show that experimentals averaged $3,636 while controls averaged $3,562, for a $74 impact on this source of income. If sample members who did not receive AFDC had been excluded from the calculations, the average total AFDC income for all experimentals or controls would have been much higher than those presented here.

A. Public Assistance Impacts for Selected Subgroups

A story begins to emerge when the sample is split into the three key subgroups: JOBSTART impacts on receipt of AFDC, Food Stamps, and General Assistance for women who were not custodial mothers (usually referred to as "other women" earlier in the report), custodial mothers, and men are presented in Tables 6.2, 6.3, and 6.4.

1. Women who were not custodial mothers. For this subgroup, Table 6.2 shows that a lower percentage of experimentals than controls ever received each type of benefit during the entire four-year follow-up period and during many of the individual years, with the impacts on AFDC receipt in the later years of follow-up being statistically significant.[5] Similarly, experimentals received less income from each source – a difference of $775 for AFDC, $340 for Food Stamps, and $209 for General Assistance – with the impact on each outcome very near the usual levels of statistical significance.

Noteworthy are the relatively large and significant differences in the AFDC receipt rates during follow-up years two, three, and four. These impacts (8.0, 9.1, and 8.6 percentage points) amounted to percentage reductions in AFDC receipt of 27 percent in both years two and three and 22 percent in year four. Impacts on welfare receipt of this magnitude are unusual in evaluations of employment and training programs and are encouraging evidence that JOBSTART may have served as a welfare prevention program for this subgroup.

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[5] The small sample size of this subgroup made it unlikely that even substantial differences would be statistically significant under the usual tests.

TABLE 6.2

IMPACTS ON AFDC, FOOD STAMPS, AND GENERAL ASSISTANCE THROUGH MONTH 48, FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN) AT THE TIME OF RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Ever received AFDC (%)				
Years 1-4	38.0	45.1	-7.1	0.120
Ever received AFDC (%)				
Year 1	12.6	14.9	-2.3	0.466
Year 2	22.0	29.9	-8.0**	0.043
Year 3	24.1	33.2	-9.1**	0.032
Year 4	30.7	39.3	-8.6*	0.056
Total AFDC income ($)				
Years 1-4	3,204	3,979	-775	0.117
AFDC income ($)				
Year 1	312	308	4	0.966
Year 2	604	795	-191	0.150
Year 3	1,001	1,311	-310*	0.082
Year 4	1,287	1,564	-277	0.159
Ever received Food Stamps (%)				
Years 1-4	58.1	64.4	-6.3	0.109
Ever received Food Stamps (%)				
Year 1	33.6	36.5	-2.9	0.411
Year 2	40.6	44.9	-4.3	0.238
Year 3	35.2	40.9	-5.7	0.151
Year 4	44.8	50.5	-5.7	0.168
Total Food Stamp income ($)				
Years 1-4	2,094	2,434	-340	0.102
Food Stamp income ($)				
Year 1	469	523	-54	0.430
Year 2	445	514	-69	0.267
Year 3	515	611	-96	0.182
Year 4	665	786	-121	0.138
Sample size	279	242		

(continued)

TABLE 6.2 (continued)

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Ever received General Assistance (%)				
Years 1-4	10.1	13.5	-3.4	0.178
Ever received General Assistance (%)				
Year 1	7.0	7.6	-0.5	0.784
Year 2	6.2	6.4	-0.2	0.912
Year 3	2.9	5.8	-2.9*	0.098
Year 4	3.9	5.9	-2.0	0.277
Total General Assistance income ($)				
Years 1-4	301	510	-209	0.106
General Assistance income ($)				
Year 1	120	122	-2	0.953
Year 2	65	66	-1	0.959
Year 3	56	154	-98*	0.052
Year 4	60	167	-107*	0.053
Sample size	279	242		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcomes, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The levels of receipt of benefits for individual years reveal the changing circumstances of these young women's lives. In the initial year of follow-up, 12.6 percent of experimentals and 14.9 percent of controls received AFDC; 33.6 percent of experimentals and 36.5 percent of controls received Food Stamps; and slightly more than 7 percent of both groups received General Assistance. In the three succeeding years, increasing percentages of both groups received AFDC and Food Stamps, but the increase in receipt was smaller for the experimental group, leading to differences in the percentage receiving AFDC (for example, 39.3 percent for controls versus 30.7 for experimentals in year four) and – to a lesser extent – Food Stamps and General Assistance. The impacts on benefit income received during the individual years reflect the same pattern, with larger welfare savings in the two later years. On a monthly basis, the pattern is also present, with fewer experimentals than controls receiving AFDC in most months (not shown in the table).

2. Custodial mothers. Because of their parenting status at random assignment, custodial mothers were more likely to receive public benefits than other women in the sample. For example, among custodial mothers more than 60 percent of experimentals received AFDC and Food Stamps in the first year of follow-up and more than 80 percent received them at some point within the four-year period (see Table 6.3). Except for impacts on the percentage receiving Food Stamps in years two and three, none of the impacts on the percentage receiving a specific benefit was either large or statistically significant. In the case of Food Stamps, there was also an abrupt change in the direction of impacts between years two and three: 6.1 percent fewer experimentals than controls received Food Stamps in year two, but by the following year 7 percent *more* received them. In general, there is not any clear trend over time in these impacts.

Over the entire four years of follow-up, Table 6.3 indicates that there was virtually no difference in AFDC payments to custodial mothers between experimentals and controls, although the annual differences that emerged in years three and four are somewhat encouraging. Over the four years, the experimentals received an average of $112 more than the controls in Food Stamps ($4,872 versus $4,760) and $359 more for General Assistance ($744 versus $385). The experimental-control differential was statistically significant for General Assistance, but not for Food Stamps. The statistically significant experimental-control differences in General Assistance payments in years three and four ($123 and $164, respectively) were the primary reason for the large difference over the 48 months.

3. Men. The public benefits analyzed in this section were much less important for men than women, with about 10 percent or less of the men ever receiving AFDC or General Assistance, and less than half ever receiving Food Stamps. Table 6.4 presents the four-year and annual impacts

TABLE 6.3

IMPACTS ON AFDC, FOOD STAMPS, AND GENERAL ASSISTANCE THROUGH MONTH 48, FOR WOMEN LIVING WITH THEIR OWN CHILD(REN) AT THE TIME OF RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Ever received AFDC (%)				
Years 1-4	84.8	81.6	3.2	0.318
Ever received AFDC (%)				
Year 1	65.6	61.2	4.4	0.262
Year 2	75.5	74.3	1.2	0.737
Year 3	57.6	57.5	-0.5	0.919
Year 4	59.3	60.5	-1.2	0.801
Total AFDC income ($)				
Years 1-4	9,371	9,334	37	0.952
AFDC income ($)				
Year 1	2,167	2,072	95	0.558
Year 2	2,402	2,279	123	0.471
Year 3	2,310	2,343	-33	0.874
Year 4	2,493	2,641	-148	0.515
Ever received Food Stamps (%)				
Years 1-4	84.5	88.3	3.9	0.195
Ever received Food Stamps (%)				
Year 1	60.9	63.4	-2.5	0.504
Year 2	71.3	77.5	-6.1*	0.093
Year 3	71.1	64.1	7.0*	0.078
Year 4	74.6	71.6	3.0	0.442
Total Food Stamp income ($)				
Years 1-4	4,872	4,760	112	0.710
Food Stamp income ($)				
Year 1	849	947	-98	0.201
Year 2	982	1,090	-108	0.195
Year 3	1,401	1,251	150	0.163
Year 4	1,639	1,472	167	0.154
Sample size	249	242		

(continued)

TABLE 6.3 (continued)

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Ever received General Assistance (%)				
Years 1-4	13.6	12.9	0.7	0.815
Ever received General Assistance (%)				
Year 1	6.8	6.6	0.2	0.938
Year 2	8.3	7.5	0.8	0.713
Year 3	6.9	4.0	2.9	0.153
Year 4	8.1	5.3	2.7	0.227
Total General Assistance income ($)				
Years 1-4	744	385	359**	0.025
General Assistance income ($)				
Year 1	205	176	29	0.656
Year 2	79	36	43	0.192
Year 3	210	87	123*	0.051
Year 4	249	85	164**	0.018
Sample size	249	242		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcomes, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 6.4

IMPACTS ON AFDC, FOOD STAMPS, AND GENERAL ASSISTANCE THROUGH MONTH 48, FOR MEN

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Ever received AFDC (%)				
Years 1-4	6.2	6.2	0.0	0.987
Ever received AFDC (%)				
Year 1	1.2	1.6	-0.4	0.657
Year 2	1.8	2.6	-0.7	0.456
Year 3	3.7	3.4	0.3	0.824
Year 4	4.9	3.1	1.8	0.181
Total AFDC income ($)				
Years 1-4	386	334	52	0.667
AFDC income ($)				
Year 1	35	34	1	0.953
Year 2	27	77	-50	0.132
Year 3	153	106	47	0.357
Year 4	170	118	52	0.364
Ever received Food Stamps (%)				
Years 1-4	43.3	41.7	1.6	0.581
Ever received Food Stamps (%)				
Year 1	25.1	26.0	-0.9	0.717
Year 2	32.4	29.9	2.5	0.360
Year 3	19.5	18.6	0.9	0.717
Year 4	24.4	23.2	1.2	0.655
Total Food Stamp income ($)				
Years 1-4	1,423	1,333	90	0.570
Food Stamp income ($)				
Year 1	374	377	-3	0.953
Year 2	344	326	18	0.699
Year 3	321	288	33	0.519
Year 4	384	342	42	0.461
Sample size	434	432		

(continued)

TABLE 6.4 (continued)

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Ever received General Assistance (%)				
Years 1-4	10.5	7.9	2.6	0.151
Ever received General Assistance (%)				
Year 1	6.1	2.9	3.2**	0.014
Year 2	5.3	4.9	0.4	0.773
Year 3	2.9	2.9	-0.1	0.961
Year 4	4.0	3.6	0.4	0.752
Total General Assistance income ($)				
Years 1-4	234	268	-34	0.622
General Assistance income ($)				
Year 1	100	56	44*	0.092
Year 2	46	59	-13	0.587
Year 3	37	70	-33	0.195
Year 4	51	83	-32	0.272
Sample size	434	432		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcomes, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

on welfare receipt rates and payments for young men. JOBSTART had almost no significant[6] or substantial impacts on these three types of welfare receipt and income for this subgroup.

B. Impacts on Receipt of Other Public Benefits

Table 6.5 shows that for the full sample JOBSTART had no significant impacts during the four-year follow-up period on the receipt of public housing, Heat Assistance, Unemployment Insurance benefits, Medicaid care, or Supplemental Security Income (SSI). There was a statistically significant but small impact on receipt of Workers' Compensation: 4.7 percent of controls received this benefit during follow-up, compared to 3.1 percent of experimentals. There were no notable exceptions to this general pattern among the three key subgroups (not shown in the table).

II. Impacts on Pregnancy, Childbirth, and Marriage, Among Women

Young women who delay starting a family generally have a greater chance of acquiring a higher level of education and consequently enhancing their opportunities in the labor market than their peers who have children. Further, young women who are unmarried and become mothers have a higher probability of being dependent on welfare. Over half of all AFDC expenditures go to maintain households in which the mother was a teenager when her first child was born. In 1990, taxpayers spent an estimated $25 billion, an increase of $3.5 billion from 1989, on mothers who began families in their teens.[7] These expenditures were in the form of AFDC, Food Stamps, and Medicaid payments. Society stands to benefit if pregnancy and childbearing among young women are postponed, since this postponement may lead to an overall increase in their educational attainment and skills needed to help them escape from poverty.

Table 6.6 summarizes the four-year impacts of JOBSTART on pregnancy and births for women who were custodial mothers at random assignment and for women who were not ("other women" in the table). Among all custodial mothers, the rates of pregnancy and childbirth (shown in the top panel of the table) were higher for experimentals than controls, and the differences were large and statistically significant. It is important to note that the birth rate for controls was itself high: 57.9 percent of custodial mothers in the control group had a subsequent birth during follow-up, compared to 67.8 percent of experimentals. Looking behind the higher birth rate for experimentals among

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[6] The only exceptions were a significant 3.2 percentage point experimental-control difference in the rate of General Assistance receipt in the first year after random assignment, and a significant $44 difference in General Assistance income in that year.

[7] Center for Population Options, 1992.

TABLE 6.5

IMPACTS ON OTHER PUBLIC BENEFITS THROUGH MONTH 48

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	p[a]
Ever received public housing				
Years 1-4	30.8	30.6	0.1	0.946
Ever received Workers' Compensation				
Years 1-4	3.1	4.7	-1.6*	0.071
Ever received Heat Assistance				
Years 1-4	21.7	22.3	-0.6	0.718
Ever received Unemployment Insurance benefits				
Years 1-4	8.2	7.6	0.6	0.631
Ever received a Medicaid card				
Years 1-4	57.2	55.5	1.7	0.372
Ever received Supplemental Security Income (SSI)				
Years 1-4	1.8	2.6	-0.8	0.260

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcome, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. Sample sizes for outcomes varied from 960 to 984 for experimentals and 928 to 940 for controls.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 6.6

IMPACTS ON PREGNANCY AND CHILDBIRTH THROUGH MONTH 48, BY PARENTAL STATUS AT THE TIME OF RANDOM ASSIGNMENT

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
All custodial mothers at random assignment				
Ever pregnant, years 1-4 (%)	76.1	67.5	8.6**	0.032
Ever gave birth, years 1-4 (%)	67.8	57.9	9.9**	0.024
Sample size	255	249		
Custodial mothers never married at random assignment				
Ever pregnant, years 1-4 (%)	75.2	70.5	4.7	0.296
Ever gave birth, years 1-4 (%)	66.5	60.0	6.5	0.178
Sample size	208	198		
Custodial mothers ever married at random assignment				
Ever pregnant, years 1-4 (%)	79.6	56.1	23.5**	0.037
Ever gave birth, years 1-4 (%)	73.0	50.3	22.7*	0.066
Sample size	47	51		
Other women				
Ever pregnant, years 1-4 (%)	64.4	65.6	-1.2	0.781
Ever gave birth, years 1-4 (%)	52.7	56.5	-3.9	0.380
Sample size	278	247		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcomes, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

custodial mothers, the two middle panels of the table reveal that the birth rate impact was largest for custodial mothers who were married at random assignment, though it was also positive (though smaller and not significant) for those who were unmarried. The experimental-control differences of 23.5 and 22.7 percentage points for pregnancy and childbirth rates, respectively, for married custodial mothers were statistically significant. Despite these large impacts, because of the relatively small number of women who were married custodial mothers at random assignment, almost two-thirds of births to all custodial mothers during the follow-up period occurred among unmarried custodial mothers.

Among women who were not custodial mothers at random assignment (shown in the bottom panel of the table), 56.5 percent of the control group had given birth by the end of the follow-up period, compared to 52.7 percent of the experimental group, a difference that was not statistically significant. There was also a small and statistically insignificant reduction in the pregnancy rate for this subgroup.

These varying impacts on birth rate for the two subgroups of women are related to the findings already presented about impacts on AFDC receipt and income. One likely reason for the lower AFDC income of experimentals among "other women" is their slightly lower rate of childbirth as compared to controls, coupled with positive earnings impacts in later years. (AFDC grants decline as income increases.) Among custodial mothers, the higher birth rate for experimentals balanced out their positive earnings impacts, leading to no impact on AFDC income.

On the related issue of marital status (not shown in the table), JOBSTART had no impact on the proportion of young people who had ever married by the end of the four-year follow-up period. Approximately 75 percent of both the experimental and control groups remained unmarried, with slightly more than 15 percent of both groups married and living with a spouse by the end of follow-up. There were no impacts on marital status for either subgroup of women; for males, experimentals were slightly more likely to have gotten married, though the difference was insignificant.

III. Impacts on Criminal Activity and Drug Use

Currently there is great public concern about criminal activity and drug use, especially among low-income males. The JOBSTART Demonstration provided an opportunity to learn more about the levels of criminal activity and drug use among low-income young people, as well as the impact of an education and training program on such activities. Questions about criminal activity are relevant when analyzing the JOBSTART sample of disadvantaged youths, especially the young men, since almost half of the males were arrested during the four-year follow-up period.[8]

A. Criminal Activity

In evaluating a job training program such as JOBSTART, one might hypothesize that when opportunities for participants to be employed become greater, involvement in illegal activities becomes less attractive. The expectation is that fewer crimes and fewer arrests will occur, reducing the burden on the judicial system and taxpayers.

Table 6.7 presents impacts on arrests in the first year of follow-up and over the entire four years. JOBSTART produced a small but significant reduction in arrests for the full sample during the first year of follow-up (2.6 percentage points), when most experimentals were active in the program, but there was no statistically significant impact over the entire four-year period. The pattern for the key subgroups shown in the table was in most cases similar: a small (and in one case statistically significant[9]) in-program decline in arrests, followed by a post-program period when the impact disappeared. The exception to this pattern is an important one: For males who had been arrested between age 16 and random assignment, the in-program impact was virtually zero, and in the remainder of the follow-up period controls appeared to be somewhat more likely to be arrested than experimentals.[10]

These impacts were more modest than those found in the previous study of the residential Job Corps program. Males in the Job Corps study experienced a 9.4 percentage point lower arrest rate while in the program than their comparison group, while females had a 2.5 percentage point lower rate during participation.[11] However, much of the Job Corps' impact on criminal behavior during the in-program period probably resulted from the "isolation" effect. In the residential Job Corps program, young people move from their community to a special center that provides the education and training services. These centers are often in isolated areas or communities without a large gang population or heavy drug trade activity. In JOBSTART, the young people continued to live in their neighborhood and spent time outside the program with their existing circle of friends.

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[8] In all the follow-up surveys, information was collected on arrests rather than convictions. This was done because of the frequency of plea bargains, the special treatment of young offenders by the courts, and the time lag between arrests and trials in many jurisdictions. The surveys, therefore, did not indicate whether arrests led to conviction or incarceration.

[9] The in-program impact for males without a prior arrest was 6.4 percentage points and statistically significant.

[10] "Appeared to be" because the sample size was too small to allow the difference to be statistically significant, although the absolute difference was 5.8 percentage points, or slightly over 8 percent.

[11] Mallar et al., 1978.

TABLE 6.7

IMPACTS ON ARRESTS THROUGH MONTH 48 FOR THE FULL SAMPLE AND KEY SUBGROUPS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p^a
Full sample				
Ever arrested, year 1 (%)	10.1	12.6	-2.6*	0.092
Sample size	851	779		
Ever arrested, years 1-4 (%)	29.0	29.3	-0.2	0.902
Sample size	982	942		
Men not arrested between age 16 and random assignment				
Ever arrested, year 1 (%)	11.2	17.6	-6.4**	0.037
Sample size	277	278		
Ever arrested, years 1-4 (%)	37.6	38.4	-0.8	0.830
Sample size	445	448		
Men arrested between age 16 and random assignment				
Ever arrested, year 1 (%)	35.1	35.1	-0.1	0.992
Sample size	106	82		
Ever arrested, years 1-4 (%)	68.9	74.8	-5.8	0.362
Sample size	127	109		
Women living with own child(ren)				
Ever arrested, year 1 (%)	3.4	6.3	-2.9	0.167
Sample size	221	216		
Ever arrested, years 1-4 (%)	14.3	16.0	-1.7	0.594
Sample size	256	247		
Women not living with own child(ren), including those who did not have any				
Ever arrested, year 1 (%)	3.5	4.2	-0.7	0.696
Sample size	247	203		
Ever arrested, years 1-4 (%)	12.7	12.3	0.4	0.880
Sample size	281	247		

(continued)

TABLE 6.7 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcome, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

B. Drug Use

Drug use among young people continues to have serious repercussions, although surveys indicate that the percentage of the U.S. population that has tried drugs has remained relatively constant during the past 15 years.[12] Many of the arrests discussed in the previous section were drug-related offenses, and this type of criminal activity can have long-lasting effects. Drug use among adolescents has been correlated with low educational achievement, a higher rate of infection with the AIDS virus, and an increase in violent crime.[13]

Table 6.8 provides data on drug use by experimentals and controls in the JOBSTART Demonstration during the final year of follow-up.[14] For that year, slightly fewer experimentals (16.2 percent) than controls (17.5 percent) reported ever using any drugs, but this difference was not statistically significant. More controls than experimentals reported the use of drugs other than marijuana, and the 1.7 percentage point difference was statistically significant.

Reported drug use was consistently higher among males than females: For example, more than 20 percent of males reported use of any drug in the year prior to the final survey, compared to 12 percent of females. JOBSTART led to a 2.5 percentage point decline in the use of any drug in the prior year for males (not significant), compared to no impact for females. For drugs other than marijuana, males experienced a 1.7 percentage point decline and females a 1.3 percentage point decline. For marijuana, impacts were -2.7 percentage points for males and -0.9 percentage points for females.

Impacts on drug use were strong and, in one case, significant for males with an arrest between age 16 and entry into JOBSTART. For this group, drug use was much higher than for the rest of the sample – over one-fourth reported using drugs in the prior year. Twenty-five percent of experimentals reported using any drug in the prior year, as compared to 31 percent of controls, for a difference of 5.5 percentage points (not significant). Focusing on drugs other than marijuana, 3.7 percent of experimentals and 10.5 percent of controls reported use in the prior year, resulting in a large (6.9 percentage points or 186 percent) and significant impact. Marijuana use also appeared to be lower among experimentals (by 4.9 percentage points), but again the impact was not statistically significant.

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[12] U.S. General Accounting Office, 1991.

[13] U.S. General Accounting Office, 1991.

[14] In the final survey, youths were asked about drug use during the prior year.

TABLE 6.8

YEAR-FOUR IMPACTS ON DRUG USE FOR THE FULL SAMPLE AND KEY SUBGROUPS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Full sample				
Ever used any drug in year 4 (%)	16.2	17.5	-1.2	0.469
Ever used any drug in year 4, excluding marijuana (%)	4.1	5.8	-1.7*	0.093
Ever used marijuana in year 4 (%)	14.6	16.3	-1.8	0.279
Sample size	980	938		
All men				
Ever used any drug in year 4 (%)	21.2	23.7	-2.5	0.380
Ever used any drug in year 4, excluding marijuana (%)	4.6	6.3	-1.7	0.264
Ever used marijuana in year 4 (%)	20.0	22.7	-2.7	0.326
Sample size	440	445		
Men arrested between age 16 and random assignment				
Ever used any drug in year 4 (%)	25.4	31.0	-5.5	0.386
Ever used any drug in year 4, excluding marijuana (%)	3.7	10.5	-6.9**	0.058
Ever used marijuana in year 4 (%)	25.3	30.2	-4.9	0.437
Sample size	123	109		
All women				
Ever used any drug in year 4 (%)	12.0	12.0	0.0	0.994
Ever used any drug in year 4, excluding marijuana (%)	3.9	5.3	-1.3	0.307
Ever used marijuana in year 4 (%)	9.9	10.8	-0.9	0.637
Sample size	540	493		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcomes, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

IV. Impacts on Positive Activity

A further step in understanding JOBSTART's employment and earnings impacts is to examine whether access to the program led more experimentals than controls to spend their time in "positive activity," defined as either work or further education and training. This outcome measure combines the findings from participation in education and training and employment into one composite measure.

As already discussed, experimentals initially were working less than controls because they were active in JOBSTART, and even in the later years of follow-up, continued investment in education and training by experimentals could have delayed employment "payoffs." Table 6.9 presents impacts on positive activity for the full sample, men, custodial mothers, and all other women. For the full sample and the three subgroups, over the entire 48-month follow-up period, nearly all individuals in the experimental and control groups had done something "positive" at some point, although experimentals had slightly higher rates of such activity. During the first 24 months of follow-up, a higher percentage of experimentals than controls spent time in positive activity, with a statistically significant difference for the full sample and each subgroup. But by the end of the first 24 months of follow-up, monthly percentages of experimentals and controls engaged in positive activity were approximately equal (not shown in the table). Over the final two years of follow-up, almost equal numbers of experimentals and controls were in either employment or education and training activities.

V. A Summary of JOBSTART's Subgroup Impacts

In this section, the impact findings presented in Chapters 5 (on labor market outcomes) and 6 (on non-labor market outcomes) are summarized for custodial mothers, all other women, all males, and males arrested between age 16 and program entry. Again, it is important to remember that dividing the sample into these subgroups reduces the likelihood of finding statistically significant impacts.

For the two female subgroups, JOBSTART appears to have made the most differences in the lives of women who were not custodial mothers when they entered the program. Positive earnings impacts, while not statistically significant, did appear to be present in the third and fourth years of follow-up and for the entire four-year period. Pregnancy and childbirth rates were slightly lower for experimentals than controls for this group, and AFDC receipt was clearly lower. Since all of these young women, as low-skilled school dropouts, were at risk of serious employment problems and

TABLE 6.9

IMPACTS ON POSITIVE ACTIVITY THROUGH MONTH 48, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Full sample				
Positive activity, years 1-2 (%)	97.6	85.5	12.1***	0.000
Positive activity, years 3-4 (%)	79.9	80.9	-1.0	0.558
Positive activity, years 1-4 (%)	98.6	93.9	4.7***	0.000
Sample size	988	953		
Men				
Positive activity, years 1-2 (%)	98.6	92.1	6.6***	0.000
Positive activity, years 3-4 (%)	89.0	89.2	-0.2	0.920
Positive activity, years 1-4 (%)	99.5	97.2	2.3***	0.007
Sample size	448	452		
Women living with own child(ren)				
Positive activity, years 1-2 (%)	95.5	73.9	21.7***	0.000
Positive activity, years 3-4 (%)	69.9	71.8	-1.9	0.638
Positive activity, years 1-4 (%)	97.5	89.4	8.1***	0.000
Sample size	257	251		
Women not living with own child(ren), including those who did not have any				
Positive activity, years 1-2 (%)	97.5	85.6	11.9***	0.000
Positive activity, years 3-4 (%)	74.9	74.8	0.1	0.980
Positive activity, years 1-4 (%)	97.9	92.8	5.2***	0.005
Sample size	283	250		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data on the specific outcomes, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

subsequent welfare receipt, these findings are encouraging signs that JOBSTART could serve as a welfare prevention program.

The impact story for custodial mothers and males is less encouraging. For custodial mothers, there were positive – though not significant – earnings impacts in the third and fourth years and for the entire four-year period, but higher pregnancy and childbirth rates for experimentals and no decline in welfare receipt. For males, experimentals remained behind controls in four-year earnings because of large initial losses during the in-program period. Furthermore, there were few encouraging impact findings on the other outcomes analyzed in the chapters.

However, for males with a prior arrest the findings are encouraging. Earnings impacts were large and significant, and began to appear early in the follow-up period. Further, there were large and significant impacts on drug use and some signs that JOBSTART may have reduced post-program arrests. Although caution is in order when reviewing these findings owing to the small sample size for this subgroup, JOBSTART appears to have made a clear positive difference for these young men, whose serious barriers to employment and troubled lives are a matter of great public concern.

CHAPTER 7

THE COSTS AND BENEFITS OF JOBSTART

This chapter presents a comparison of the costs and benefits of the JOBSTART program. It draws on program cost information presented in Appendix C and on the impact data in Chapters 5 and 6 of this report. The benefits and costs are assessed from the perspectives of program participants, taxpayers, and society as a whole.[1] This analysis attempts to summarize the main program impacts discussed in earlier chapters and to provide a framework for answering several fundamental questions regarding JOBSTART's achievements. Within the benefit-cost framework, the benefits produced by JOBSTART's addition of services above those received by controls are compared to the cost of these added services.

Among other topics, this chapter examines whether the added services provided by JOBSTART had a net payoff for participants, and if so, when during the follow-up period such a payoff began. In calculating net program effects, participants' earnings, welfare benefits, and program-related transfer payments are combined into a summary measure, describing the program's overall measured economic consequences for participants. This analysis is done for JOBSTART's full 48-month impact sample, as well as for the three key subgroups: men, women who were custodial mothers at entry into the program, and all other women in the sample.

An important aspect of any benefit-cost analysis is the influence of time on the final conclusions about cost-effectiveness. The period considered in this analysis begins at the point when participants and funders invested their resources, and extends over the four-year follow-up period. For the initial investment to pay off, future benefits must exceed it by a certain margin, representing the time value of money and the effect of inflation. By discounting benefits to the initial in-program year, the benefit-cost analysis addresses the question of the program's real value over time, in this case the four-year follow-up period.

It must be noted that many questions remain unanswered in this benefit-cost analysis. In its attempt to combine different program effects into a summary measure, the analysis excludes many program effects that cannot be expressed in dollar terms. For instance, in Chapter 4, JOBSTART was shown to have substantial impacts on the receipt of education services and subsequent educational attainment. Aside from their indirect effects on earnings and welfare receipt, these education impacts are not represented in this analysis, even though additional, non-monetary benefits for participants and taxpayers may be associated with these education gains.

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[1] The taxpayer perspective is not entirely appropriate in the context of the JOBSTART Demonstration, which was funded in part with private contributions. However, if JOBSTART was expanded into a more large-scale program, taxpayers would probably be the primary funders and benefits would accrue to them. The funders' perspective is therefore referred to in this report as the taxpayers'. Given certain assumptions regarding the distribution of costs and benefits – especially that benefits and costs to participants and taxpayers should be weighted equally – the taxpayer and participant perspectives can be combined into a "societal" perspective.

The first of the six sections in this chapter describes the analytical framework. The second begins by presenting the program's gross costs[2] and continues with a discussion of the receipt of non-JOBSTART services by the control and experimental groups, and an estimation of the net cost of JOBSTART. Section III presents costs and benefits from the perspective of program participants. These costs and benefits are based on the impacts discussed in Chapters 5 and 6 and the cost analysis in Section II. Section IV includes a benefit-cost comparison from the taxpayers' perspective and concludes with an analysis that combines aspects of both participant and taxpayer perspectives to determine the payoff of JOBSTART to society as a whole. Some of the non-monetary effects of JOBSTART are considered in Section V in relation to the benefit-cost analyses presented earlier, and the chapter concludes in Section VI with a summary.

I. Analytical Approach

This benefit-cost analysis uses an approach similar to that followed in MDRC's previous evaluations of job training and welfare employment programs.[3] The analysis combines the dollar values of the program's measured effects and its use of resources into a net present value. Non-monetary program effects can be considered in comparisons of the program's benefits and costs. Once monetary benefits and costs have been calculated and compared, the reader can assess whether non-monetary factors are likely to be large enough to change the initial conclusion. The effects and resources included in this analysis are listed in Table 7.1, which also indicates their expected general value (positive, negative, or zero) from the three perspectives considered.

The resources include all JOBSTART operating costs, regardless of funding source. They also include compensation to participants for program-related expenses as well as the cost of support services. Both experimentals and controls received non-JOBSTART education and training services during the four-year follow-up period. The effect of the use of these non-JOBSTART resources

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[2] As mentioned above, this analysis uses Appendix C as its primary data source.
[3] See, for instance, Hamilton and Friedlander, 1989, p. 91.

TABLE 7.1

THEORETICAL VALUE OF COMPONENTS OF THE JOBSTART BENEFIT-COST ANALYSIS, BY ACCOUNTING PERSPECTIVE

	Accounting Perspective		
Component	Program Participants	Taxpayers	Society
Increased earnings and fringe benefits	+	0	+
Increased tax payments			
Payroll taxes	–	+	0
Income and sales taxes	–	+	0
Reduced use of transfer programs			
AFDC payments	–	+	0
Food Stamp payments	–	+	0
General Assistance payments	–	+	0
Payments from other public programs	–	+	0
AFDC administrative costs	0	+	+
Food Stamp administrative costs	0	+	+
Reduced use of community education and training programs	0	+	+
Reduced criminal activity and income	–	+	+
JOBSTART operating costs	0	–	–
Compensation for program-related expenses	0	–	–
Additional support services	+	–	0
Change in family's financial needs owing to program effects on childbearing	?	?	?
Value of education not reflected in earnings	+	+	+
Preference for work over welfare	+	+	+
Foregone leisure time and activities	–	0	–

NOTE: The components are shown as a theoretical benefit (+), cost (–), or neither a benefit nor a cost (0), according to a priori expectations regarding their value.

on the *net* program costs is discussed in Section IIB below. The monetary program impacts considered include those on earnings and welfare receipt, discussed in Chapters 5 and 6, as well as effects on tax payments, Food Stamps, and administrative costs associated with transfer programs. Table 7.1 also includes non-monetary benefits such as the value of education not reflected in earnings and a preference for work over welfare, and non-monetary costs such as foregone leisure time.

Whether a given program effect or the use of specific resources is a benefit or a cost depends on what is actually measured and the analytical perspective that is taken. Table 7.1 shows components of the benefit-cost analysis and the expected value of each component from the participant, taxpayer, and societal perspectives. Each plus and minus in Table 7.1 reflects the expected benefit or cost status of a component assuming that the program is successful, but the actual result may be different. Once measured, particular effects or expenses are valued as a benefit, a cost, or irrelevant, depending on which of the three perspectives is considered.

The program participants' perspective identifies the benefits and costs for members of the experimental group, indicating how they fared as a result of JOBSTART. This perspective is particularly important in a comparison of program benefits to opportunity costs for participants. The taxpayers' perspective identifies benefits and costs from the standpoint of everyone in society *other* than individuals who received program services. For example, if JOBSTART reduced AFDC receipt among its participants, that effect would translate into a loss for participants but a gain for taxpayers. The societal perspective assesses whether the program's benefits exceed its costs when the participant and taxpayer perspectives are combined.

II. Program Costs

An extensive analysis of JOBSTART program costs was performed to identify the market value of *all* resources used in providing JOBSTART services. It therefore includes as program costs not only the expenses incurred by the agencies sponsoring the program, but also those incurred by outside organizations responsible for providing certain services, such as occupational skills training in the three sequential/brokered sites.

During the JOBSTART Demonstration, data were collected from a variety of sources containing information on fixed and variable operating costs, support services, and participation rates. In most cases, data from these sources covered a one-year "steady-state" period between 1985 and 1988, the years during which JOBSTART was funded. The year-long period selected in most sites began at least several months after the initiation of the project (in order to avoid including the start-up costs associated with beginning a new program) and ended at least several months prior to the termination of the demonstration (in order to exclude the phaseout period).

A. The Gross Costs of JOBSTART

The average total cost of JOBSTART per experimental was determined by summing the average cost of several relatively distinct program components and services (see Table 7.2, which contains weighted averages for all sites).[4] The table shows that basic education was the largest cost component of the program, with over 22 percent of total expenses, followed by occupational skills training, with 20 percent of total expenses. Another 19 percent of program expenses were dedicated to program coordination and counseling. The fact that the program focused primarily on human capital development is apparent in the relatively low level of spending on labor force attachment services, such as job development and work-readiness skills training, which together made up less than 13 percent of the total expenses.

The average cost per experimental for the delivery of the JOBSTART services (intake, education, training, job placement assistance, counseling, and life skills workshops) varied widely across the sites (see Table 7.3, under "Service Delivery"). Although it fell between $4,000 and $5,500 in most sites, it ranged from slightly over $2,000 in CET/San Jose, EGOS in Denver, and SER/Corpus Christi to a high of about $7,500 in BSA in New York City.[5] Calculating the cost per participant instead of per experimental can change the picture somewhat. In particular, the cost per experimental at CET/San Jose was substantially lower than the cost per participant at the site because only 64 percent of experimentals there were ever active in JOBSTART, as compared to the all-site average of 89 percent. Consequently, on a *per-participant* basis, EGOS in Denver had the lowest service delivery cost among the sites.

As discussed in Chapter 3, JOBSTART also offered support services and small stipends, which varied by site and were for transportation, child care, and other services and expenses related to participation in JOBSTART. The total cost of these benefits varied from a low of $61 at CET/San Jose to a high of $1,389 at the Los Angeles Job Corps site. In the majority of sites, the payments and support services averaged between $200 and $600 per experimental. In Table 7.3, this category

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[4] The figures in Table 7.2 were calculated by multiplying the expenses per experimental reported by the sites for each program component by the number of experimentals in the site, and dividing the total by 988 (the number of experimentals in the 48-month sample).

[5] For a discussion of the sources of these figures, see Appendix C.

TABLE 7.2

COSTS OF JOBSTART, BY PROGRAM COMPONENT

Component	Cost per Experimental In $	Cost per Experimental As a % of the Total Cost
Service delivery		
Recruitment, intake, and orientation	496	10.9
Basic education	1,014	22.3
Occupational skills training	915	20.1
Work-readiness or life skills training	328	7.2
Job development and placement assistance	235	5.2
Counseling and program coordination	875	19.2
Support services	568	12.5
Subtotal for core components	4,431	97.4
Medical/dental services	117	2.6
Total	4,548	100.0

SOURCE: Appendix C.

NOTES: The cost estimates are based on a sample of 949 experimentals for whom there were 24 months of follow-up survey data in 1991. These estimates are weighted averages of the site-specific figures, calculated by dividing the sum-product of these figures by 988, the number of experimentals for whom there were 48 months of follow-up survey data.

All costs are in 1986 dollars.

TABLE 7.3

COSTS OF JOBSTART, BY SITE AND PROGRAM COMPONENT

		Cost per Experimental				
Site	Sample Size	Service Delivery ($)	Support Services ($)	Subtotal for Core Components ($)	Medical/ Dental Services ($)	Total ($)
Concurrent						
Atlanta Job Corps	33	3,376	797	4,173	690	4,863
CET/San Jose	84	1,973	61	2,034	0	2,034
Chicago Commons	40	6,114	363	6,477	0	6,477
Connelley (Pittsburgh)	91	4,738	447	5,185	0	5,185
East LA Skills Center	50	4,800	87	4,887	0	4,887
EGOS (Denver)	103	1,609	467	2,076	24	2,100
Phoenix Job Corps	70	4,059	897	4,956	357	5,313
SER/Corpus Christi	125	1,505	593	2,098	0	2,098
Sequential/in-house						
El Centro (Dallas)	93	4,794	512	5,306	0	5,306
LA Job Corps	116	4,161	1,389	5,550	564	6,114
Sequential/brokered						
Allentown (Buffalo)	71	5,128	734	5,862	0	5,862
BSA (NYC)	60	7,279	205	7,484	0	7,484
CREC (Hartford)	52	4,934	232	5,166	0	5,166
All sites	988	3,863	568	4,431	117	4,548

SOURCE: Appendix C.

NOTES: The cost estimates are based on a sample of 949 experimentals for whom there were 24 months of follow-up survey data in 1991. The "all sites" estimates are weighted averages of the site-specific figures, calculated by dividing the sum-product of these figures by 988, the number of experimentals for whom there were 48 months of follow-up survey data.

All costs are in 1986 dollars.

of expenses is presented separately from service delivery costs and medical/dental costs to allow them to be treated differently depending on the analytical perspective.[6] In addition to the usual support services, some sites provided extensive medical benefits,[7] which must be considered as transfer payments from taxpayers to participants. A more extensive discussion of program costs, and a detailed breakdown by site of the costs of services provided, is included in Appendix C.

As shown in Table 7.3, the average total cost per JOBSTART experimental was $4,548,[8] of which $3,863 (85 percent) was spent to operate the program, $568 (12 percent) on support services, and $117 (3 percent) on medical and dental services.[9] In the United States, the average annual cost per pupil for public education was $3,839 for the 1985-86 school year,[10] the first year of the JOBSTART program operation. JOBSTART was a remedial program for high school dropouts and can be considered an attempt to offset the negative effects of missing two school years, or the equivalent of a $7,600 public investment.[11] Other programs serving a comparable target group are JTPA and the Job Corps. The former, which is less intensive than JOBSTART, spent less than $2,000 per new enrollee in program year 1986.[12] The latter, which offers more intensive services than either JTPA or JOBSTART, including providing residential services in addition to education and training, cost $10,545 per year per participant in the mid-1980s.[13]

B. The Net Costs of JOBSTART

As presented earlier in this report, 56 percent of control group members received remedial or occupational instruction in a program other than JOBSTART, while 94 percent of experimental group members received JOBSTART and non-JOBSTART services. The program's impacts are therefore the *incremental* impacts of the services received by experimentals over the mix of services available to these youths outside of JOBSTART (represented by the services received by controls). For the calculation of a *net* program cost estimate, the costs of these control services must be subtracted from the gross program costs presented earlier. However, experimentals also received non-JOBSTART services, which they usually pursued on their own after leaving JOBSTART *but* during the 48-month follow-up period. The costs of these services should also be included in the net cost estimate.

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[6] In this analysis, some support services are treated as transfer payments to participants and some as program expenses that did not affect participants' income directly.

[7] Only the Atlanta Job Corps, EGOS in Denver, the Phoenix Job Corps, and the Los Angeles Job Corps offered these benefits.

[8] This overall average cost estimate is a weighted mean of the average costs by site. The weighting was done by multiplying each site's cost per experimental by the number of experimentals in the site and dividing the result by 988.

[9] The mean of $117 includes values of zero for the sites where these benefits were not offered.

[10] U.S. Department of Education, National Center for Education Statistics, 1989.

[11] The average dropout grade at baseline was 10 for experimentals in JOBSTART: that is, the average JOBSTART participant lost two years of high school.

[12] U.S. Congress, House Ways and Means Committee, 1992, p. 1692.

[13] U.S. General Accounting Office, 1986.

Table 7.4 shows that 94 percent of all experimentals in the 48-month impact sample received education or training from JOBSTART or non-JOBSTART sources, compared to 56 percent of all controls, for a difference of 38 percentage points. The table also shows that almost all of the experimental-control difference in service receipt is accounted for by the JOBSTART services received by the experimentals: 363 hours of the 367-hour difference in average service receipt were JOBSTART education and training services received by experimentals. This results from the fact that experimentals and controls received approximately equal average hours of education and training from non-JOBSTART sources (437 hours for experimentals versus 432 for controls).[14] Therefore, the difference between experimentals and controls in service receipt is approximately equal to the amount of JOBSTART services received by experimentals.

Since the non-JOBSTART services for experimentals and controls were approximately equal, the analysis in this chapter assumes that the *net* cost of providing education and training services to the experimentals is simply the cost of providing JOBSTART services. In other words, in this special circumstance, the gross cost of providing JOBSTART services is also the net cost of the program.

III. The Participant Perspective

From the perspective of the participant, the effect of a program such as JOBSTART is the effect of all financial gains and losses combined, regardless of the source of these gains and losses. In other words, income from employment is treated in the same manner as income from public sources. Therefore, the participant perspective offers a measure of the *net* effect of any program-induced earnings gains on the participant's disposable income because welfare losses are taken into account.

As shown in Chapter 5, JOBSTART slightly increased participants' earnings over the four-year follow-up period. This increase is not significant for the full sample or for any of the three key

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[14] The probability of experimentals and controls receiving non-JOBSTART training services differed by only 1.7 percentage points (39.7 percent for experimentals versus 38 percent for controls), and although controls were more likely to receive educational services from non-JOBSTART providers, this did not lead to a difference in total hours of services received from non-JOBSTART providers.

TABLE 7.4

IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 48, BY TYPE OF SERVICE PROVIDER

Outcome and Service Provider	Experimentals	Controls	Difference
Ever received education or training from any provider[a] (%)	94.0	56.1	37.9
Ever received education from (%)			
JOBSTART	85.5	0.0	85.5
Other providers	32.8	39.8	-7.0
Ever received training from (%)			
JOBSTART	66.5	0.0	66.5
Other providers	39.7	38.0	1.7
Total hours of education or training received from[a]			
JOBSTART	363	0	363
Other providers	437	432	4
All providers	800	432	367
Sample size	988	953	

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

Tests of statistical significance were not performed.

[a] A breakdown by type of service received was not available.

subgroups. However, the point estimates for the earnings effects in each of the four follow-up years can be used to assess the program's costs and benefits to the participants. As discussed in Chapter 5, participation in a voluntary program such as JOBSTART is likely to entail some opportunity cost for the participants in the earlier part of the follow-up period. Participants in JOBSTART invested their time and effort to improve their skills, expecting this investment to pay off in the future. In the meantime, they may have given up the opportunity to work and acquire new skills on the job.

The first test of JOBSTART's success is whether it was a useful investment for the participants. The impacts, presented in Chapter 5, show that the program raised earnings for experimentals above those for controls by a statistically insignificant $214 over the full follow-up period. Underlying this impact is a time pattern showing losses in the first two years, which were offset by gains in the last two. These earnings gains, amounting to $423 and $410 in years three and four, respectively, are almost statistically significant (respective p-values of 0.102 and 0.125).[15]

In addition to these earnings impacts, program participants experienced additional program effects, some of which cannot be readily expressed in dollar terms. Among the effects that can be expressed in dollar terms, and that directly affected the participants' disposable income, are impacts on a number of important public benefits. Chapter 6 showed that for the full sample no impacts on these benefits were either large or statistically significant: Overall, there was little or no difference in the amount of income that experimentals and controls received in the form of AFDC, Food Stamps, and General Assistance over the four-year follow-up period. Since the magnitude of these welfare impacts is very small and some are positive and some negative, their net effect on the program's benefits for participants is very small. However, as suggested by Chapter 6, the effects for the subgroups (presented below) are somewhat different.

As mentioned earlier, other program benefits that must be included in the analysis are support services that were made available to participants while they were in the program. Many of these support services were aimed at reducing direct costs associated with individuals' participation in the program, most notably transportation and child care expenses. Because the actual amount of such costs to participants are not measured explicitly, offsetting benefits (that is, the support services they received) will not be counted either. However, some support services did more than compensate for direct costs related to participation in the program. The best example of these are the extensive health care services that were offered at some sites, which may have constituted a significant benefit to the participants, one that was not available to controls. The average value of this benefit per experimental was $117. Since most program participation occurred in the first year after random assignment, this benefit will be counted then. Other support services will be considered compensation for unmeasured costs of participation and will not be counted as benefits to the participant.

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[15] To the extent that earnings gains are accompanied by increased tax liability, such liability is a loss to participants. No tax losses were included in this analysis, although it can be assumed that some occurred.

Table 7.5 and Figure 7.1 show a summary of the program's costs and benefits as perceived by the participants. For each of the four follow-up years and for the full four-year period, the table presents selected impact estimates from Chapters 5 and 6 (except for medical/dental benefits, for which the cost estimate from Appendix C is presented). As its bottom line, the table also presents the net present value of the four-year impact on each outcome measure, which is adjusted for inflation and foregone returns from alternate uses of program resources.[16]

It is important to note that the four-year net present value includes some estimates that are not statistically significant. In other words, one cannot be very confident that these impacts would differ from zero if the JOBSTART Demonstration were replicated. For this reason, among others, the net present value results based on the measured impacts should be viewed as *estimates*, the precision of which is uncertain. Nonetheless, they do represent the best available evidence of JOBSTART's observable economic consequences.

As Table 7.5 shows, the program's net present value to participants is quite small, $254, but positive, indicating that the program had begun to break even by the end of the follow-up period. Figure 7.1 shows *cumulative* program effects over the four years of follow-up.[17] The program's payoff did not start to materialize until after the third year, but the initial gap between experimentals and controls disappeared entirely by the end of the follow-up period.

The trend underlying this graph points toward a larger future payoff for experimentals, but the data on which these impacts are based cannot be used to confirm the continuation of this trend. Future results will depend on the extent to which the program's impacts in the latter two years are sustained over time.

Table 7.6 and Figure 7.2 show the program's estimated net present value for participants in the three key subgroups. Four years after random assignment, only women who were custodial mothers

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[16] The rates of inflation for the second, third, and fourth years of follow-up were 3.6, 4.1, and 4 percent, respectively (based on the consumer price index from the U.S. Bureau of Labor Statistics). To this, a rate of 5 percent for foregone returns from alternate uses of program resources was added, bringing the final cumulative discount percentages for years two through four to 8.6, 9.1, and 9.8 percent, respectively.

[17] The graph was created by adding together the impacts in each year and subsequently discounting them. The discounted figures were then plotted on the graph.

TABLE 7.5

THE PARTICIPANTS' PERSPECTIVE: SELECTED IMPACTS AND ESTIMATED NET PRESENT VALUES THROUGH MONTH 48

Follow-Up Period	Earnings and Fringe Benefits[a] ($)	AFDC ($)	Food Stamps ($)	General Assistance ($)	Medical/Dental Benefits[b] ($)	Total ($)
Year 1	-559	63	-45	24	117	-400
Year 2	-136	24	-42	7	0	-147
Year 3	474	-3	31	-6	0	496
Year 4	459	-11	31	3	0	482
Years 1-4	240	74	-24	29	117	432
Net present value of 4-year impact	69	74	-34	28	117	254

SOURCES: MDRC calculations from JOBSTART enrollment form and survey data (impact figures); Appendix C (medical/dental benefit figures).

NOTES: Impact calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. See Appendix C for the sample used for the medical/dental cost estimates.

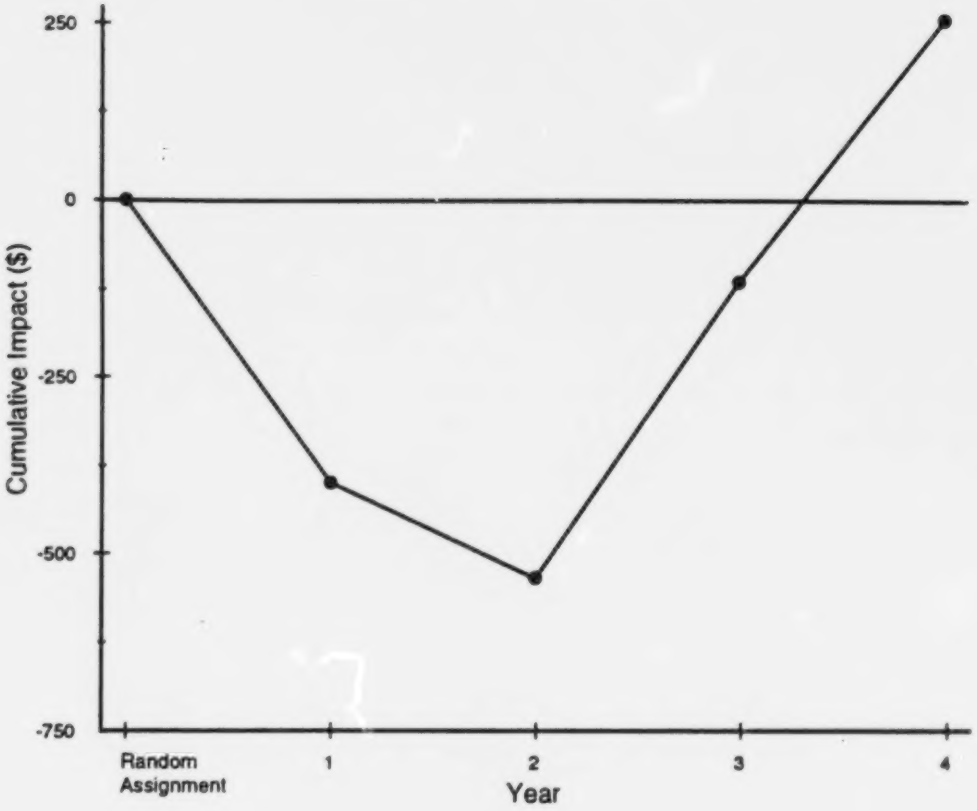
In calculating the estimated net present value, all costs and benefits in years two through four were discounted to 1986 dollars at a rate of 5 percent plus the rate of inflation as measured by the consumer price index.

[a] Fringe benefits were calculated at 12 percent of reported earnings.

[b] Medical/dental benefits were estimated using accounting reports on experimentals' receipt of these benefits (see Appendix C). Controls were not eligible for them. For accounting purposes these benefits will be assumed to have occurred in the first year after random assignment.

FIGURE 7.1

THE PARTICIPANTS' PERSPECTIVE:
CUMULATIVE DISCOUNTED IMPACTS
THROUGH MONTH 48



SOURCE AND NOTE: **This graph shows the cumulative net present value of the annual impacts in Table 7.5.**

TABLE 7.6

THE PARTICIPANTS' PERSPECTIVE: SELECTED IMPACTS AND ESTIMATED NET PRESENT VALUES THROUGH MONTH 48, BY GENDER AND PARENTAL STATUS

Subgroup and Follow-Up Period	Earnings and Fringe Benefits[a] ($)	AFDC ($)	Food Stamps ($)	General Assistance ($)	Medical/Dental Benefits[b] ($)	Total ($)
Men						
Year 1	-909	1	-3	44	117	-750
Year 2	-444	-50	18	-13	0	-489
Year 3	497	47	33	-33	0	544
Year 4	551	52	42	-32	0	613
Years 1-4	-306	52	90	-34	117	-81
Net present value of 4-year impact	-475	35	74	-20	117	-270
Women living with own child(ren)						
Year 1	-161	95	-98	29	117	-18
Year 2	168	123	-108	43	0	226
Year 3	367	-33	150	123	0	607
Year 4	325	-148	167	164	0	508
Years 1-4	700	37	112	359	117	1,325
Net present value of 4-year impact	553	67	57	298	117	1,093
Women not living with own child(ren), including those who did not have any						
Year 1	-384	4	-54	-2	117	-319
Year 2	85	-191	-69	-1	0	-176
Year 3	470	-310	-96	-98	0	-34
Year 4	516	-277	-121	-107	0	11
Years 1-4	687	-775	-340	-209	117	-520
Net present value of 4-year impact	488	-646	-292	-168	117	-501

(continued)

TABLE 7.6 (continued)

SOURCES: MDRC calculations from JOBSTART enrollment form and survey data (impact figures); Appendix C (medical/dental benefit figures).

NOTES: Impact calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. See Appendix C for the sample used for the medical/dental cost estimates.

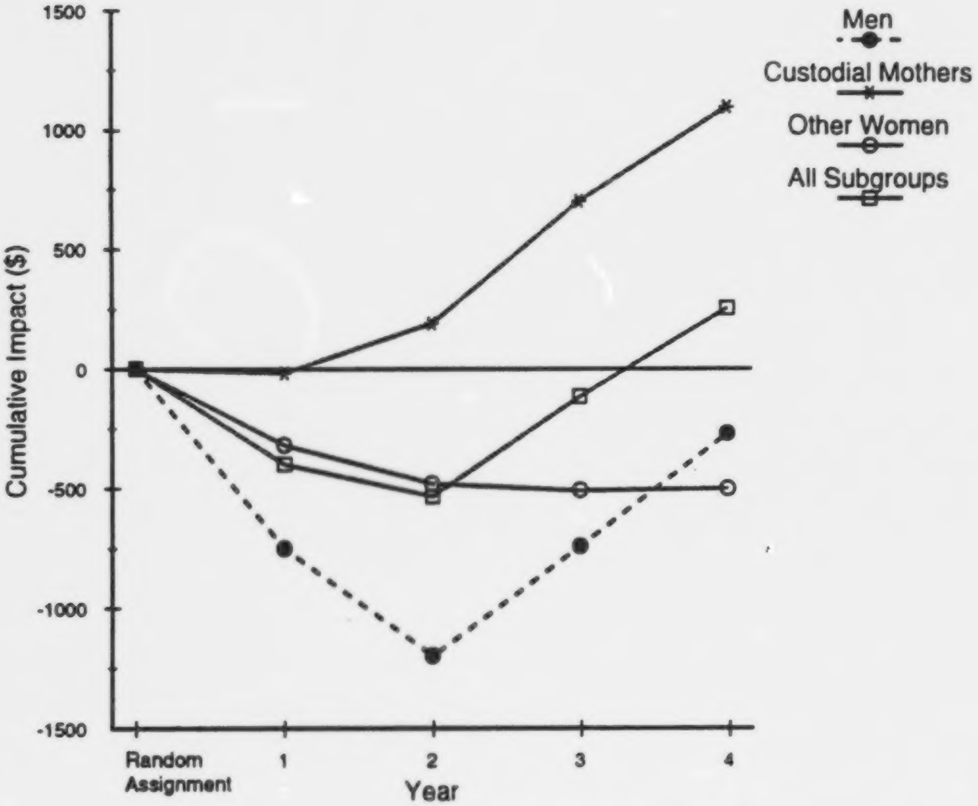
In calculating the estimated net present value, all costs and benefits in years two through four were discounted to 1986 dollars at a rate of 5 percent plus the rate of inflation as measured by the consumer price index.

[a] Fringe benefits were calculated at 12 percent of reported earnings.

[b] Medical/dental benefits were estimated using accounting reports on experimentals' receipt of these benefits (see Appendix C). Controls were not eligible for them. For accounting purposes these benefits will be assumed to have occurred in the first year after random assignment.

FIGURE 7.2

THE PARTICIPANTS' PERSPECTIVE:
CUMULATIVE DISCOUNTED IMPACTS THROUGH MONTH 48,
BY GENDER AND PARENTAL STATUS



SOURCES AND NOTE: This graph shows the cumulative net present value of the annual impacts in Tables 7.5 and 7.6.

at entry into the program experienced a positive net present value, slightly exceeding $1,000. The other two subgroups, men and all other women, were not better off after four years; the program resulted in a negative net present value for them. The net present value for men was -$270; for all other women it was -$501.

The negative results for males are explained mostly by initial foregone earnings among participating experimentals. This opportunity cost of the program amounts to a loss of earnings and fringe benefits of more than $1,350 in the first two years after random assignment. Their later gains were insufficient to offset this loss.

The major reason for JOBSTART's unfavorable effects on the net income of other women is that JOBSTART reduced the amount of transfer payments they received compared to their control group counterparts. This, in turn, appears to reflect JOBSTART's impact on birth rates for this group. As shown in Chapter 6, women in the experimental group who were not custodial mothers at random assignment were somewhat less likely to become pregnant or give birth during the follow-up period than similar women in the control group. This would make them less likely to become eligible for or need programs such as AFDC and Food Stamps. To the extent that negative impacts on transfer income are explained by lower birth rates, the loss of this income is offset by a reduction in the costs of supporting a family. From a financial point of view, such a reduction in the cost of having and raising children implies that the reduced transfer payments are not really experienced by these women as an economic *loss*.

A similar point can be made about the program's apparent lack of impact on public assistance receipt for women who were custodial mothers at random assignment. Chapter 6 showed that for this subgroup there was a positive impact on further childbearing. However, many of these births appeared to occur among married women, which may explain why no significant impact on welfare receipt was recorded for this subgroup, since AFDC, the most common type of public assistance, is provided primarily to women who are single parents.

In conclusion, it appears that for the full 48-month impact sample the program reached the break-even point for experimentals at the end of the four-year follow-up period. For custodial mothers, this break-even point was reached much earlier, at roughly 18 months after random assignment. Consequently, for this group the payoff at the end of the observation period was much larger, slightly more than $1,000. Men, who after four years still faced a negative net present value of $270, appeared to be nearing the point at which the program would begin to pay off. However, the extent of the ultimate payoff for men will probably be smaller than that for custodial mothers, since ongoing future benefits will be worth less and less as a result of inflation and discounting to determine a present value. Finally, for women who were not custodial mothers at random assignment, the costs of the program appeared to outweigh its benefits. After four years, no financial turnaround had yet occurred for these women, who faced a $501 loss despite encouraging earnings gains. Looking only at the program's effect on family income, these women will benefit financially from JOBSTART only if earnings impacts continue to grow in the future. But it is important to remember that JOBSTART led to smaller households for this subgroup (owing to fewer births), so the available income supports fewer people.

Finally, the reader must be reminded that the costs and benefits addressed in this section are limited to *easily measurable, financial* program effects. There were many other program impacts of JOBSTART (presented in Chapters 4 and 6), and to the extent that impacts on outcomes such as educational attainment and family status affect other aspects of experimentals' lives or affect earnings beyond the observation period, the program's full impact may not be captured in this analysis.

IV. Other Benefit-Cost Perspectives

A. The Taxpayer Perspective

As expected, the program's benefits to taxpayers in relation to the full 48-month impact sample were very limited (see Table 7.7 and Figure 7.3). In the absence of any real welfare savings for the full sample, no payoff in reduced government spending occurred to offset the $4,500 investment made in each JOBSTART experimental. Table 7.8 and Figure 7.4 show the findings from the taxpayer perspective for the three main subgroups. Note that no tax gains were included in this analysis, even though it can be assumed that participants' earnings gains did result in somewhat higher tax payments.

The lack of substantial welfare savings for the full sample is related to the target group and goal of the program. JOBSTART, in contrast to other welfare-to-work programs, was not targeted specifically at welfare recipients. Also, welfare receipt rates for men and for women who were not custodial mothers at baseline were low. The only welfare savings of some note, for women who were not custodial mothers at entry into the program, appear to have resulted more from lower birth rates than from increased employment.[18] However, it is possible that for this and other subgroups

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[18] Note that savings in administrative costs, associated with reductions in welfare payments, were not included in this analysis.

TABLE 7.7

THE TAXPAYERS' PERSPECTIVE: SELECTED IMPACTS AND ESTIMATED NET PRESENT VALUES THROUGH MONTH 48

Follow-Up Period	Net Cost of Operating JOBSTART[a] ($)	AFDC ($)	Food Stamps ($)	General Assistance ($)	Total ($)
Year 1	-4,548	-63	45	-24	-4,590
Year 2	0	-24	42	-7	11
Year 3	0	3	-31	6	-22
Year 4	0	11	-31	-3	-23
Years 1-4	-4,548	-74	24	-29	-4,624
Net present value of 4-year impact	-4,548	-74	34	-28	-4,616

SOURCES: MDRC calculations from JOBSTART enrollment form and survey data (impact figures); Appendix C (operating cost figure).

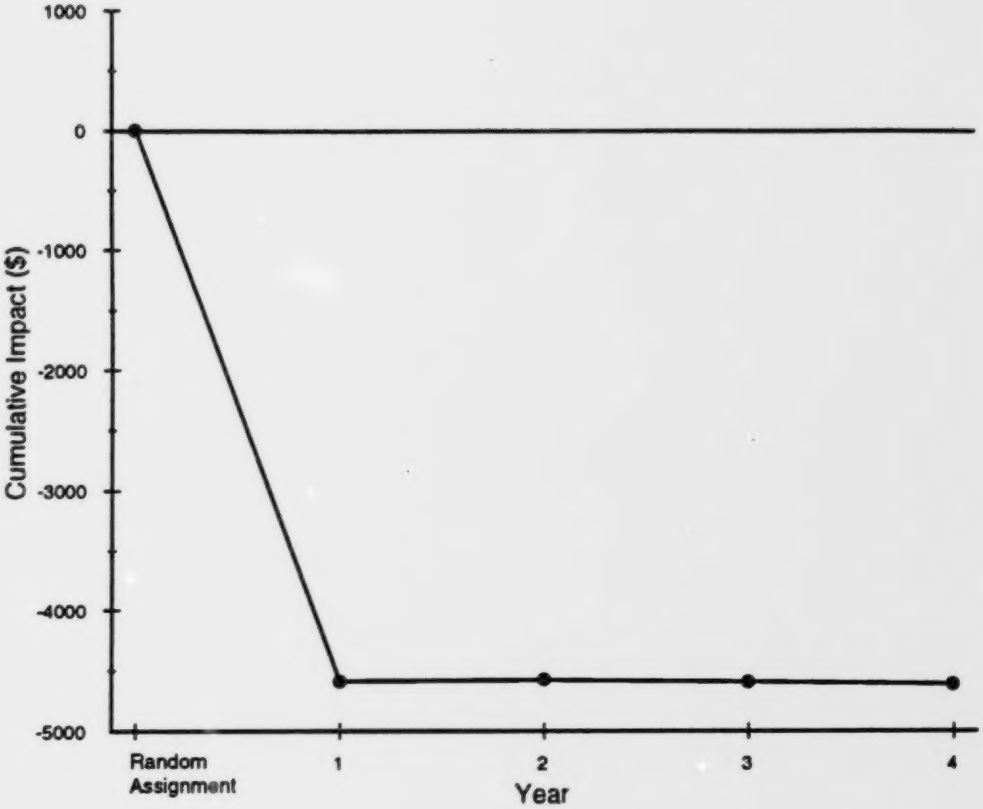
NOTES: Impact calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. See Appendix C for the sample used for the operating cost estimate.

In calculating the estimated net present value, all costs and benefits in years two through four were discounted to 1986 dollars at a rate of 5 percent plus the rate of inflation as measured by the consumer price index.

[a] The method of calculating the JOBSTART operating cost is described in Appendix C. This cost includes medical/dental benefits provided to participants. For accounting purposes this cost will be assumed to have occurred in the first year after random assignment.

FIGURE 7.3

THE TAXPAYERS' PERSPECTIVE: CUMULATIVE DISCOUNTED IMPACTS THROUGH MONTH 48



SOURCE AND NOTE: This graph shows the cumulative net present value of the annual impacts in Table 7.7.

TABLE 7.8

THE TAXPAYERS' PERSPECTIVE: SELECTED IMPACTS AND ESTIMATED NET PRESENT VALUES THROUGH MONTH 48, BY GENDER AND PARENTAL STATUS

Subgroup and Follow-Up Period	Net Cost of Operating JOBSTART[a,b] ($)	AFDC ($)	Food Stamps ($)	General Assistance ($)	Total ($)
Men					
Year 1	-4,548	-1	3	-44	-4,590
Year 2		50	-18	13	45
Year 3		-47	-33	33	-47
Year 4		-52	-42	32	-62
Years 1-4	-4,548	-52	-90	34	-4,539
Net present value of 4-year impact	-4,548	-35	-74	20	-4,636
Women living with own child(ren)					
Year 1	-4,548	-95	98	-29	-4,574
Year 2		-123	108	-43	-58
Year 3		33	-150	-123	-240
Year 4		148	-167	-164	-183
Years 1-4	-4,548	-37	-112	-359	-4,939
Net present value of 4-year impact	-4,548	-67	-57	-298	-4,971
Women not living with own child(ren), including those who did not have any					
Year 1	-4,548	-4	54	2	-4,496
Year 2		191	69	1	261
Year 3		310	96	98	504
Year 4		277	121	107	505
Years 1-4	-4,548	775	340	209	-3,107
Net present value of 4-year impact	-4,548	646	292	168	-3,442

(continued)

TABLE 7.8 (continued)

SOURCES: MDRC calculations from JOBSTART enrollment form and survey data (impact figures); Appendix C (operating cost figure).

NOTES: Impact calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. See Appendix C for the sample used for the operating cost estimate.

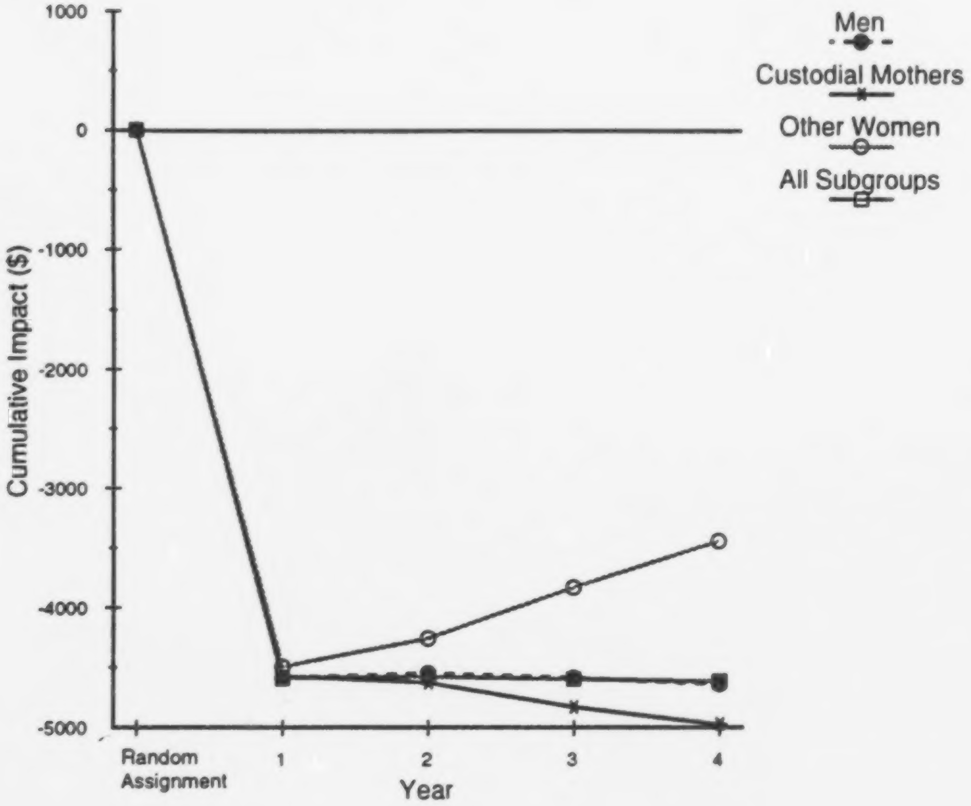
In calculating the estimated net present value, all costs and benefits in years two through four were discounted to 1986 dollars at a rate of 5 percent plus the rate of inflation as measured by the consumer price index.

[a]The method of calculating the JOBSTART operating cost is described in Appendix C. This cost includes medical/dental benefits provided to participants. For accounting purposes the cost will be assumed to have occurred in the first year after random assignment.

[b]Average length of stay and hours of participation in JOBSTART varied among the three subgroups in this table; therefore, the cost estimate must be considered an approximation of the real program expenses for each subgroup.

FIGURE 7.4

THE TAXPAYERS' PERSPECTIVE:
CUMULATIVE DISCOUNTED IMPACTS THROUGH MONTH 48,
BY GENDER AND PARENTAL STATUS



SOURCES AND NOTE: This graph shows the cumulative net present value of the annual impacts in Tables 7.7 and 7.8.

additional benefits will accrue over a period of time beyond the follow-up period, as a result of the increased educational attainment resulting from participation in the program.

B. Combining the Participant and Taxpayer Perspectives

Even when the many uncertainties surrounding this benefit-cost analysis are taken into account, it is likely that, after four years, JOBSTART had taken much more in resources for program participation and operations than it had produced in benefits to participants, taxpayers, or society as a whole. The final figure for society (-$4,286; see Table 7.9) indicates that the marginal gains for program participants ($254) do little to offset the large losses to taxpayers ($4,540). Note that this figure was obtained simply by summing the final figures for participants and taxpayers in Table 7.9.

Implicit in this method of calculating a "societal" benefit-cost estimate is the assumption that a dollar for participants is equal in value to a dollar for taxpayers, that is, both bottom-line estimates are combined with a simple weight of one. It is sometimes argued that this is not a valid approach: Since JOBSTART participants had fewer resources than the average taxpayer, the marginal value of resources provided them could be higher (although conventional economic theory is very cautious in making such assumptions). According to this view, participants' gains should be weighted more heavily than taxpayers' losses. Such differential weighting would somewhat reduce the overall loss to society. However, the bottom line for society is likely to remain negative at four years after follow-up.

V. Non-Monetary Program Effects

In Table 7.1, a number of program costs and benefits were listed that were not included in this analysis because they cannot be assigned a dollar value. Some of these effects were discussed in earlier chapters and deserve to be mentioned here, since they may help to put the benefit-cost results into perspective. They include: (1) the value of education beyond that arising from higher earnings; (2) the preference for work over welfare; and (3) the cost of foregone leisure time and activities.

The first effect relates to the remedial character of the JOBSTART program. As a program targeted at high school dropouts, one of JOBSTART's primary goals was to increase the educational level and attainment of program participants, a goal that was achieved: JOBSTART experimentals received considerably more education and training than members of the control group, and during the follow-up period many succeeded in attaining a GED or high school diploma. For the participants, this program effect had begun to pay off in increased earnings and labor force

TABLE 7.9

ACTUAL VALUE OF COMPONENTS OF THE JOBSTART BENEFIT-COST ANALYSIS, BY ACCOUNTING PERSPECTIVE

Component	Accounting Perspective		
	Program Participants	Taxpayers	Society[a]
Increased earnings and fringe benefits	$69	$0	$69
Increased tax payments			
Payroll taxes	–	+	0
Income and sales taxes	–	+	0
Reduced use of transfer programs			
AFDC payments	$74	-$74	$0
Food Stamp payments	-$34	$34	$0
General Assistance payments	$28	-$28	$0
Payments from other public programs	–	+	0
AFDC administrative costs	0	+	+
Food Stamp administrative costs	0	+	+
Reduced use of community education and training programs	0	+	+
Reduced criminal activity and income	–	+	+
JOBSTART operating costs	$0	-$3,863	-$3,863
Compensation for program-related expenses	$0	-$568	-$568
Additional support services	$117	-$117	$0
Change in family's financial needs owing to program effects on childbearing	?	?	?
Value of education not reflected in earnings	+	+	+
Preference for work over welfare	+	+	+
Foregone leisure time and activities	–	0	–
Total	$254	-$4,540	-$4,286

SOURCES: Tables 7.5-7.8.

NOTES: All costs are in 1986 dollars.

Components that have not been assigned a dollar value are shown as a benefit (+), cost (–), or neither a benefit nor a cost (0), according to a priori expectations regarding their value.

[a] In calculating the value of each component from society's perspective, the participant and taxpayer values were equally weighted.

participation by the end of follow-up, even though subsequent effects on welfare receipt proved insufficient to offset the program's cost to taxpayers. On the other hand, both participants and taxpayers may likely have benefited in other ways from these positive education impacts. Education may increase people's involvement in the political process, as well as enhance their participation in civic life. Attainment of educational credentials, especially a high school degree or GED, can open up opportunities for further education, which may not be reflected in participants' earnings for many years after the program has ended. Also, parents' educational achievement may affect their children's lives and performance in school as well.

Regarding the second effect listed above, a benefit-cost analysis from the participants' perspective may show a limited payoff from work, owing to concomitant reductions in welfare income. However, it is generally believed that former welfare recipients prefer work over welfare, even if not all of their efforts translate into higher income.[19] Further, taxpayers may also experience a non-monetary benefit from welfare savings in addition to their tax savings, since many object to providing income transfers to able-bodied individuals and prefer income to be linked to employment.

Finally, an additional program cost must be mentioned. Aside from the measured opportunity cost (i.e., reduced earnings during the first two years after random assignment), JOBSTART participants also lost valuable nonwork or "leisure" time while they were in the program or subsequently employed. Loss of this time and the resulting foregone activities are difficult to measure and value correctly; therefore, this loss was not included in the benefit-cost analysis. However, one cannot ignore these program costs, especially since what is commonly labeled "leisure time" often accommodates such highly valued activities as raising children or taking care of other family members.

VI. Conclusion

Table 7.9 presents a summary of the benefit-cost results drawn from tables and figures earlier in this chapter. Two facts become apparent from this table: (1) Many program effects were not explicitly valued in this analysis and they may have affected the results, and (2) the program's bottom line after four years of follow-up from both the taxpayer and societal perspectives appears to be largely negative. This concluding section will use both these observations in summarizing the results of the benefit-cost analysis, as well as put them in a proper perspective.

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[19] See, for example, Coalition on Human Needs, 1987.

As was noted earlier, only the most important and easily measured program effects were included in the benefit-cost analysis. Table 7.9 uses symbols (+, −, 0) to indicate the assumed, unquantified value of many components of the analysis, rather than presenting actual estimated dollar values. In some cases, these nonestimated costs and benefits are directly associated with other components that were measured and discussed in the analysis. Such is the case, for instance, with taxes (associated with earnings) and reductions in administrative costs (associated with welfare savings). Such secondary costs and benefits comprise only a fraction of the value of those with which they are associated. They were not included in this benefit-cost analysis because their overall effect on the bottom line was so small as not to warrant further investigation into their actual dollar value.

Other costs and benefits that were not measured in dollar terms cannot be dismissed so easily. The non-monetary value of education was mentioned earlier as a potentially important, yet unmeasured, benefit. It is hard to assess the value of this benefit within the observation period, and even harder to estimate its value in the longer run. It is sometimes argued that many of the benefits of education and educational attainment accrue to future generations.

Another benefit that has not been assigned a dollar value is JOBSTART's apparent impact on criminal activity. Even though Chapter 6 did not show a sustained impact on this outcome, a significant in-program effect on arrests was found (a 2.6 percent reduction in the first year). It is difficult to estimate the value of this reduction to all groups involved, especially since the type of arrest and the conviction rate were not analyzed. However, a small gain to taxpayers and society may have occurred.

Finally, for some outcomes it is hard to tell whether they are costs or benefits. Earlier, the program's effects on childbearing were discussed. It is known that childbearing in JOBSTART's target population is positively associated with welfare receipt. However, it is difficult to establish the benefit or cost of a new life to mothers, taxpayers, or society. No attempt was made to do so. Nevertheless, in interpreting the results of this benefit-cost analysis, the reader should be aware of these non-monetary program effects and their possible relationship to the effects that *were* measured and presented in dollar terms.

CHAPTER 8

THE IMPLICATIONS OF JOBSTART FOR PROGRAMS AND POLICY

This chapter moves beyond the research findings presented so far to offer suggestions about the most promising program and policy responses to the findings. Although the chapter draws primarily on the JOBSTART evaluation, it takes into account results from other recent studies as well as operational experience from many youth programs. It also incorporates key findings from nonexperimental research on JOBSTART conducted by MDRC staff, which is presented in detail in a separate technical paper.[1]

I. A Summary of the JOBSTART and Related Findings

Rigorous impact analyses of programs are complicated to conduct, so the list of studies providing solid evidence of the effects of youth employment programs is not a long one,[2] and the evidence is somewhat discouraging. A recent U.S. Department of Labor summary of youth research concluded, "The available findings suggest that employment training programs have generally had small average impacts on youth. Excluding findings from the Job Corps, there is no evidence that any of the programs has more than a modest and short-term effect."[3] Since the release of that report, interim findings on out-of-school youths from the National JTPA Study and the four-year JOBSTART findings have become available.[4]

The interim findings from the National JTPA Study provide evidence of the effectiveness of

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[1] See Cave and Bos, 1993.

[2] See Betsey et al., 1985, for a review of the research in this field as of the mid-1980s, and Smith and Gambone, 1992, and other papers in the same volume for a more recent review. Smith and Gambone present a table on "major sources of evidence regarding youth employment program effects," which includes results from only eight programs.

[3] Smith and Gambone, 1992, p. 49.

[4] Also released in 1992 were findings from longer-term follow-up for the Summer Training and Education Program (STEP), a demonstration that spanned two summers and combined paid work experience, life skills training, and education for economically disadvantaged 14- to 15-year-olds at risk of dropping out of school. Earlier reports had found promising in-program effects on academic skills and knowledge of health and contraception practices. Unfortunately, the longer-term follow-up revealed no impacts on school graduation rates, birth rates, or a long list of other outcomes. See Walker, 1992, for an overview of the demonstration and findings. Although the youths targeted in STEP were somewhat different from those in the other programs evaluated, the findings have contributed to the general pessimism about the effectiveness of programs serving young people.

Title IIA programs serving disadvantaged out-of-school youths within a diverse sample of SDAs. Based on 18 months of post-random assignment follow-up, the results show that access to JTPA increased the rate at which youths completed high school or passed the GED examination, but made little difference in the earnings of young women in general or young men without a prior arrest. Further, JTPA appears to have led to a reduction in the earnings of young men with a prior arrest. The local programs seemed to have had particular difficulty helping the more disadvantaged young men in the research sample: those who were school dropouts, received public assistance, or had not worked recently.[5] While firmer conclusions regarding the JTPA programs must await the release of longer-term findings based on 30 months of follow-up, these interim findings illustrate the challenge of designing effective youth employment programs.

It might appear that the most likely conclusion from these evaluations is that almost nothing works – that is, has enduring effects – for disadvantaged youths. (The Job Corps stands out as the sole exception.) In-program effects can be achieved – active participation in services, impacts on intermediate outcomes such as GED receipt – but there are no real long-term effects on employment, earnings, and welfare receipt once young people leave the program. But while the message from the recent research is far from optimistic, the conclusion that no program has enduring effects is clearly overly pessimistic. In JOBSTART, there were apparent earnings gains – "apparent" because they just missed the usual standards for statistical significance – in the third and fourth years of follow-up, well after the end of program services. In fact, the pattern of earnings impacts observed in JOBSTART is similar to that expected for such a program: an initial period when participants forego earnings (the "opportunity costs" of being in the program), followed by a period when they catch up with controls, followed by a period when their earnings exceed those of controls. This pattern suggests that investing in education and training – *above the level of services received by the control group* – can lead to increases in earnings over time.

The problem in JOBSTART concerned the magnitude and duration of these negative and positive earning impacts, not the lack of any payoff: The initial losses for some subgroups were too large and the post-program payoff too modest, at least during the four-year follow-up available for this study. Table 8.1 summarizes earnings impacts by year for the full sample and key subgroups. For

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[5] See Bloom et al., 1993, for the details of the findings. In interpreting them, it is important to understand that the impacts measured in the National JTPA Study – as is also the case for the JOBSTART Demonstration – represent the effect of services received by the experimental group above the level of services received by the control group. In the case of the National JTPA Study, however, the service difference was relatively modest.

TABLE 8.1

IMPACTS ON EARNINGS THROUGH YEAR FOUR
FOR THE FULL SAMPLE AND KEY SUBGROUPS

Subgroup	Sample Size	Year 1 ($)	Year 2 ($)	Year 3 ($)	Year 4 ($)	Years 1-4 ($)
Full sample	1,941	-499***	-121	423	410	214
Men						
All men	900	-812***	-396	444	492	-273
Arrested between age 16 and random assignment	237	-936	425	1,129	1,872**	2,491
Not arrested between age 16 and random assignment	663	-850***	-669*	461	178	-882
Women						
Living with own child(ren)	508	-144	150	328	290	625
Not living with own child(ren)[a]	533	-343	76	420	461	613
Reason for leaving regular high school						
School-related	925	-312	175	726*	592	1,181
Job-related	197	-1,108	220	254	405	-230
Other	819	-545***	-375	147	196	-578

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] Includes women who did not have children.

the full sample, a $499 earnings loss in the first year was followed by a smaller loss in year two, and gains of $423 and $410 in the third and fourth years, respectively.

Behind these results is a great variety of experience among subgroups. As mentioned in Chapter 5, most of the earnings impact estimates for subgroups were not statistically significant; however, the impacts offer suggestive evidence about how JOBSTART worked for different types of youths in the sample. For some subgroups there were encouraging apparent earnings impacts. Among males, the initial losses were much larger than the average for the full sample, but the estimated gains in the later years also appear to be somewhat larger. There was an important distinction between males who had not been arrested between age 16 and program entry and those who had been: Those with a prior arrest had a shorter period of initial earnings losses followed by larger earnings gains in years three and four (with the latter being statistically significant), resulting in apparent gains for the entire four-year period. Among females, those who were custodial mothers at random assignment had very small initial earnings losses followed by small apparent earnings gains. All other women in the sample experienced first-year earnings losses of a somewhat larger magnitude than did custodial mothers (but still much smaller than those of men), followed by increasing apparent earnings gains in years two through four. For both men and women, the reason for dropping out of school also appeared to affect earnings impacts. Young people who left school because of school-related reasons (for example, they did not like school, earned poor grades, or had behavior problems) had larger than average earnings impact estimates in the latter two years of follow-up and for the entire four years. Those who left school for job-related reasons (for example, because they preferred to work, needed a job, or wanted to learn a trade) had large initial earnings losses that could not be compensated for by the earnings gains in later years, which led to a small four-year earnings loss. The subgroup who left school for other reasons (in many cases owing to pregnancy or the birth of a child) experienced initial earnings losses during the first two years and only small earnings gains in the remaining years, resulting in a sizable – though not significant – earnings loss for the entire four-year period. Finally, the earnings impacts at one of the JOBSTART sites – CET in San Jose – stand out as much stronger and more consistent over time than those at the other 12 local programs in the demonstration.

While participants would obviously have gained more from the program if their initial earnings losses were smaller and later payoffs larger, the benefit-cost analysis presented in Chapter 7 illustrates why such improvements would also have been important to taxpayers and society as a whole. The substantial investment of resources to provide JOBSTART services requires decreases in experimentals' initial earnings losses and/or increases in their later earnings for the program to be cost-effective in a conventional benefit-cost framework.

Operating JOBSTART within the JTPA context of the late 1980s was a challenge, and the resulting funding uncertainties and impediments to serving low-achieving school dropouts in a low-cost, placement-driven system no doubt took their toll on the program. As JTPA programs have moved closer to the JOBSTART model – most notably through the dropping of the cost performance standard, and through the recent amendments that create a separate year-round youth title within JTPA and call for more targeting of youths with multiple employment barriers and the provision of more intensive services – many of the institutional barriers to operating programs such as JOBSTART will lessen. If JOBSTART had operated under these new JTPA regulations, the implementation challenges would have been less severe.

The remainder of this chapter presents ideas based on research findings and operational experience about how to change JOBSTART and similar programs to take advantage of this more hospitable environment and improve earnings impacts. It offers suggestions that should be relevant to both current JTPA programs and others. The discussion is divided into three parts: suggestions on targeting program recruitment, options for lessening the initial earnings losses (the opportunity cost of participation), and ways of increasing the earnings payoff in later years.

II. Targeting Program Outreach

As discussed in the previous section, in JOBSTART there was substantial variation among subgroups in the extent of initial foregone earnings and the size of later earnings gains. The subgroup findings provide evidence that a program such as JOBSTART – which had the capacity to provide intensive education and training – has the best effect if youths with relatively serious employment barriers are recruited to participate. The more promising initial employment prospects of youths with fewer barriers to employment are likely to produce large initial earnings losses that will be hard to compensate for in later years.

However, two cautions are necessary in interpreting this conclusion. First, programs less intensive than JOBSTART may have a different pattern of subgroup impacts. In the National JTPA Study, for example, the 18-month earnings impacts for both males and females tended to be better for those who were *more* job-ready. The relatively short-term interventions funded by JTPA in the study sites did not result in large opportunity costs, and the services may not have been intensive enough to help the less job-ready clients. Therefore, because different programs may be more or less effective for different subgroups, no single targeting rule can be applied across the board.[6]

Second, the nature of the program experience can change if *all* participants face serious barriers to employment; without some youths who are relatively employable, there will be fewer role models and success stories to help motivate youths and provide satisfaction for staff. This implies that the key lesson to draw from the subgroup findings is that a substantial percentage of participants should have serious barriers to employment, but that programs should strive for a mixture of skills levels among youths served. As the percentage of harder-to-serve clients increases, program managers should more closely monitor the achievement of intermediate and long-term milestones by participants and the morale and motivation of the young people and staff.

The pattern of subgroup impacts found in JOBSTART suggests the following outreach strategies for similar programs:

- **Young women.** The findings for young women who were not custodial mothers when they entered JOBSTART – especially the reductions in their AFDC receipt rate and in the amount of AFDC received – suggest that program operators should work to recruit this group. This will require outreach in the community rather than reliance on referrals from other social service or public assistance agencies, because most of these women are not receiving any type of public assistance and few have ties to public or nonprofit agencies.

- **Males with a prior arrest.** Programs can establish links to the justice system to aid recruiting, and might even start participation in the program before the end of incarceration or while young people are on probation. Further, participation might be made a condition for early release for those in prison, or the program could operate as an alternative to incarceration for first-time offenders.

- **Young people who dropped out for school-related reasons.** Recruiting young dropouts into an employment program such as JOBSTART may be easiest if the target is youths actively seeking a job or training, but the subgroup findings imply that programs should seek ways to identify young people who dropped out for school-related reasons. It may be possible to cooperate with local school counselors and teachers to find ways to inform young people who leave school for such reasons about the alternative to the traditional educational system provided by programs such as JOBSTART. For example, programs could get lists from school counselors of young people who have recently dropped out owing to educational difficulties and make contact with them right away.

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[6] Interestingly, in the National JTPA Study, impacts for males were worse for those with a prior arrest, the opposite of the JOBSTART findings. It could be that during enrollment in a government-funded employment and training program, staff learn whether experimentals have an arrest record and, if so, feel obligated to inform potential employers, while controls may not inform employers if they have been arrested. And although JOBSTART was operated principally with JTPA funds (that is, government funds), the impact differences for men with a prior arrest may be attributable to the fact that the short-term interventions of the JTPA study sites were not substantial enough to overcome this barrier to employment, while the JOBSTART services were.

III. Options for Combating the Initial Earnings Losses for Participants

For some young people, especially males, participating in JOBSTART resulted in substantial opportunity costs in terms of foregone earnings. The lessons on targeting program outreach, discussed above, can reduce this problem somewhat, but it will still be present for some youths. These initial opportunity costs can undermine efforts to keep young people in the program long enough for them to substantially improve their skills. There is nonexperimental evidence (cited later in this chapter) that there may be a threshold level of participation above which earnings impacts are larger, and operational experience suggests that providing youths with income while participating in a program can improve retention.[7] But even for those with substantial participation, initial earnings losses can sometimes overwhelm earnings gains in the years following program participation, making the participants worse off financially, contrary to the goal of the program.

There is no single obvious way to address the problem of initial earnings losses; all approaches pose trade-offs for program designers and operators and have budget implications. The following options provide a range of responses beyond the targeting strategies already discussed. The possibilities mentioned are not necessarily consistent with one another, but could be appropriate in different circumstances depending on funding and operational constraints. The options include: providing income during program participation and restructuring the duration and sequence of program services.

A. Linking Education and Training with Paid Work Experience

If structured properly and offered as part of a program of education and training, paid work experience has the potential – not yet carefully tested – to improve later program impacts in addition to its obvious value in providing income during program participation.[8] This argument hinges on several hypotheses. First, the young people's experiences on the job can become part of the program, serving to make the education and training more relevant and to reinforce their learning of skills through application in a real-world setting. Much recent literature has pointed out the advantages of such contextual or experiential learning.[9] Second, work experience can help the young people to be socialized into the adult world of work in a gradual, nonthreatening way and bring them into contact with adult role models in a work setting. Third, there is growing evidence that young people value opportunities to make a contribution to their community and that such service can change the way they see themselves and relate to others, and the way others see them.[10] And fourth, since many young people become involved in programs because they are seeking a job, paid work experience could lead them to stay in the program longer, learning more academic and occupational skills.

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[7] One of the attractions for young people in the YouthBuild program is the opportunity for paid work experience. This program, discussed in more detail later in the chapter, has shown strong early success in retaining young people in activities.

[8] In the Job Corps, paid work experience is an important part of the program, which also includes education and training. Research has indicated that work experience alone does not appear to be effective for very disadvantaged youths. In the National Supported Work Demonstration, a program of paid work experience was tested for several groups, including very disadvantaged young school dropouts. For these youths, the program did not increase long-term earnings and was not cost-effective. However, a follow-up test of work experience combined with education for young dropouts showed more encouraging program participation, though lack of funding prevented analysis of program impacts. See MDRC Board of Directors, 1980, for a summary of the National Supported Work Demonstration, and Scharfman, 1981, for a discussion of the special variation of supported work combined with education for young dropouts.

The new JTPA amendments and regulations make paid work experience somewhat easier to provide. Work experience in the public or nonprofit sector is permitted for youths when it is accompanied by "other services designed to increase the basic education and/or occupational skills of the participant," as would be the case in programs such as JOBSTART. Further, other income-providing activities such as cooperative education placements or "limited internships" may be arranged in private, for-profit firms. On-the-job training, which is now more restricted under JTPA, is also an option.

In considering work experience and related options, however, it is important to understand the responsibilities that providing such services entails for program operators. Developing a large number of placements may be challenging and demanding, especially when the clients are young people with low educational attainment and limited work histories. In addition, it is necessary to monitor the nature of the work and the young person's performance on the job to ensure that the employment experience is meaningful and linked to classroom training. Such monitoring entails an administrative burden when many employers are involved. Finally, in a slow-growing economy – as in recent years – wages may have to be partly or even fully subsidized, raising the costs of the service.[11]

These factors create incentives to keep the management and supervision of paid work experience in-house within a youth employment agency or in specially created organizations, as in Supported Work or YouthBuild. This increases the chance that employers will understand the initial skills and needs of the young people and will take seriously their responsibility to make the employment a useful learning experience. To avoid reinforcing unproductive work behavior, programs and employers must provide young people with a chance to do real work, set clear standards on the job, and not accept weak excuses for poor performance.

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[9] See Berryman and Bailey, 1992.

[10] Several innovative programs offer young people a chance to make a contribution to their community while learning important skills. Among them are state and local youth conservation corps programs and YouthBuild.

[11] There has been one large-scale program providing work experience to a large number of youths. The Youth Incentive Entitlement Pilot Projects (YIEPP), operated between 1978 and 1980 in 7 large and 10 smaller locales, were a test of a full-scale job entitlement program for economically disadvantaged youths. In the demonstration, 76,000 youths were employed in work experience positions and many program implementation issues were successfully overcome. See Gueron, 1984.

B. Restructuring the Duration and Sequence of Program Services

This option for lessening the opportunity costs of program participation can be accomplished through either of two almost opposite means: concentrating intense participation in a short time period or allowing for extended and less intense participation to enable youths to work while enrolled. The choice of approach would depend on the type of young people served.

1. Concentrating program participation in a short period. In JOBSTART, the site with the strongest earnings impacts (CET/San Jose) had a relatively short average length of stay in the program, but much higher than average hours of participation per month. While the length of participation is probably not the sole reason for CET/San Jose's strong performance, the site's short-term, intensive program minimized the time that young people were out of the labor market, which reduced the opportunity cost of participating. This option should be distinguished from a call for short-term, low-intensity programs; rather, the approach is more similar to a full-immersion program.

However, there are trade-offs here as well. An intense and relatively short immersion in education and training requires that youths participate more hours per day than was typical in JOBSTART sites. Also, other activities such as life skills training must be downplayed despite their apparent value for some participants. Youths who will not or cannot invest many hours per day in participation would be inappropriate for such a program. Such youths may (1) lack the interest or ability to focus for extended periods at the point when they enter the program; (2) have other responsibilities that prevent full-day program participation (for example, child care or care of another household member[12]); or (3) need to work to cover their living expenses. The first problem could be addressed by making the program sufficiently engaging, the second by providing appropriate support services, and the third by offering paid work experience. However, even these special efforts would not help some youths make the necessary commitment.

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[12] In JOBSTART, the CET/San Jose sample included a relatively low percentage of young mothers because the site was simultaneously operating the Minority Female Single Parent (MFSP) Demonstration, which targeted this group. In that study, CET/San Jose also produced strong earnings impacts.

2. Combining work and program participation over an extended period. This approach could be appropriate for youths who want or need to work at least part-time.[13] For them, a concentrated program of education and training would not be appealing. Services would have to be structured to allow participation to vary over time as the mix of education, training, and work shifted. Such programs would be making a long-term commitment to their clients and would expect skills and employment options to change gradually. In effect, the approach involves recognizing what is often a frustration for program operators (the "on and off" nature of many youths' participation in programs) and designing program services to facilitate less intense but extended participation. For relatively employable youths, unsubsidized work could be possible, while for those with substantial barriers to employment, paid work experience positions would be needed.

The challenges for program operators using this approach are substantial. It requires flexibility to allow for part-time participation, which could involve scheduling some activities at night and permitting open entry and exit in activities or individualized curricula. It also requires staying in touch with inactive participants so they know that the program will welcome them back when their work schedule or personal situation permits them to participate again. These requirements all increase the management burden on program staff.

There is also the possibility that involvement in the program would never become intense enough to make any real difference in the youths' skills or attitudes. Further, in part-time programs it is more difficult to develop the type of peer support and program cohesiveness that can develop in more intensive programs.

Unfortunately, there is no simple solution to the problem of initial earnings losses. Efforts to counteract these losses can increase program costs and – absent substantial increases in the magnitude of later earnings gains – can reduce the chance that the program will be cost-effective for funders or society. Thus, efforts to combat initial opportunity costs for participants should be coupled with efforts to improve later earnings impacts, as discussed below.

IV. Options for Increasing Long-term Payoffs

This section discusses six options for raising earnings impacts in the post-program period: (1) linking program services more closely to the job market; (2) placing more emphasis on addressing the developmental needs of youths; (3) creating various means to help more people complete the program; (4) helping more participants receive a GED; (5) strengthening job placement assistance; and (6) continuing program services after the initial job placement.

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[13] It might also be appropriate for youths who need an initial period of education to raise reading and math skills high enough to participate in training. These youths would probably face serious barriers to finding unsubsidized jobs, so paid work experience would be an important service for them.

A. Strengthening the Link Between Education and Training and the Job Market

The JOBSTART guidelines called for sites to provide training in occupations in demand in the local job market, but not all sites were able to do so. CET/San Jose, the site with the strongest earnings impacts in the study, was the most effective in involving employers in developing the program's occupational emphasis and curriculum. Training areas were chosen carefully, based on analysis of local labor market needs. The site was also unusual in the extent to which educational services were shaped by occupational training needs and provided in an integrated way.

While the earnings impacts for CET/San Jose suggest that these strategies can be effective, it is important to raise a cautionary note because of the experience in another site. Chicago Commons was the site closest to CET/San Jose in the extent to which education and training were integrated and training was tailored to meet the current demands of the local labor market.[14] At the two-year point, earnings impacts at Chicago Commons were about the same as at CET/San Jose, but in the final two years of follow-up they deteriorated sharply, leading to negative total earnings impacts for the four-year period. Two possible explanations are that employer demand for the specialized occupations in which the youths were trained declined or that the number of workers in these occupations sharply increased and the JOBSTART youths were not prepared to adapt their skills for other occupations.[15]

Paid work experience also has the potential to strengthen the link between education and training and the labor market, by providing a workplace context in which young people can apply what they are learning in the classroom. Further, it may improve longer-term earnings impacts by giving youths access to jobs with the possibility of advancement and further employer-provided training; these types of jobs might be hard for young people to find on their own. In the Youth Incentive Entitlement Pilot Projects, for example, approximately 20 percent of participants placed in private-sector work experience positions were later hired as regular employees in the same organization.[16]

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[14] For example, many participants were trained in plastic injection molding techniques.

[15] Despite their similarities, there were many differences between Chicago Commons and CET/San Jose: Most importantly, Chicago Commons had not served youths in its training programs prior to JOBSTART, while CET had done so for many years.

[16] See Ball and Wolfhagen, 1981.

B. Addressing the Developmental Needs of Youths

Many of the young people who participate in employment and training programs have lived in relative poverty and isolation for much of their lives. One of the greatest challenges these youths face is to overcome the emotional deprivation and psychological distress that result from the many housing, child care, financial, personal safety, and other problems they confront each day.[17] For too many there is not even a knowledgeable and trusted older person to help them cope with these problems and move through the normal stages of adolescent development.

Programs must help youths address these issues and teach them the interpersonal skills necessary for life at work and at home.[18] These include the ability to communicate with different types of people clearly, to work productively in a group, to make plans and carry them out, and to handle effectively the unexpected events that inevitably crop up in daily life.

In JOBSTART, many sites gradually realized the importance of addressing these life skills issues directly and increased their emphasis on them. In recent years, teaching materials for life skills development have been published and many programs, even within JTPA, now include group sessions on these topics. The recent JTPA amendments and regulation revisions recognize achievement of life skills competencies as part of program performance standards.

One approach to help young people learn these interpersonal skills is to emphasize youth leadership development: that is, give them serious responsibilities within the program. In the most innovative cases, such as YouthBuild programs, young people work together in groups, set the rules for the group, plan its activities, and carry out the plans. The governance structure is built around the young people. In addition, YouthBuild provides an intense work experience activity – renovating housing in the local community – that gives young people the opportunity to learn to handle work-related problems in productive ways, demonstrate leadership, and derive the satisfaction and self-esteem that comes from helping others.[19] Proponents of the approach believe that through this process young people can develop the skills to address the many personal and situational problems they face both within and outside the workplace.

C. Increasing Completion of Program Activities

One of the most pressing issues for designers and operators of youth programs is understanding the relationship between program participation and program impacts. In other words, is there some threshold level of service below which impacts are negative or slightly positive and above which they are strongly positive? Despite the importance of this issue, researchers studying youth programs have had difficulty providing clear guidance.

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[17] These issues are reviewed in depth in Smith and Gambone, 1992, and other papers in the same volume.

[18] See American Society for Training and Development, 1988, for a discussion of the value placed on these skills by employers.

[19] Residential Job Corps programs also provide activities to encourage this type of development.

The experimental analysis of JOBSTART's impacts presented in previous chapters does not answer this question because the overall effect of JOBSTART on the experimentals who were offered the program is the average of impacts for subgroups who received different amounts of program services; it is possible that the program's impacts varied substantially by level of participation. JOBSTART may have had high opportunity costs in the short run but large payoffs later that accrued only to those who stayed in the program long enough to learn the skills needed to achieve the payoffs.

Solving this mystery is a challenging analytical problem.[20] A forthcoming technical paper uses a variety of nonexperimental, econometric methods to gain insight into the relationship between program participation and impacts.[21] While it does not solve the analytical problems involved in such research, the findings from the variety of methods are consistent enough to offer suggestive evidence on the issue.

The most straightforward breakdown of JOBSTART's impacts by participation in the program is to distinguish the experimentals who received services from those who did not. Because JOBSTART was a voluntary program, it is reasonable to assume that the 11 percent of experimentals who were unserved ("nonparticipants") had impacts that were precisely zero, and that the group of program participants produced the entire program effect.[22] This being the case, JOBSTART had an earnings impact of $241 on program *participants* over the four years of follow-up (the $214 impact per experimental divided by 0.89). For the final two years, the nonparticipation correction transforms an $833 impact for all experimentals into a $938 impact for program participants.[23]

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[20] The basic difficulty is that the amount of participation of a young person in the experimental group is a *post-random assignment* characteristic that is affected by observed and unobserved pre-random assignment characteristics as well as experiences in the program. This makes it hard to identify the control group counterparts of those who participated either more or less in JOBSTART.

[21] See Cave and Bos, 1993. Among the techniques used by the authors to identify the control group counterparts of subgroups of experimentals are instrumental variables, two-stage least squares, and a repeated matching process.

[22] Hence, the impacts for *participants* (as opposed to all experimentals) are calculated by dividing the overall impact for experimentals by the percentage of experimentals who actually participated in program services. In JOBSTART, the participant figure is 88.8 percent for the full sample, 88.4 percent for men, 89.9 percent for women living with their own children at random assignment, and 88.3 percent for all other women. See Bloom, 1984, and Cave, 1988.

The next step is to further disaggregate JOBSTART findings for participants by their level of participation.[24] The findings from a breakdown by hours of participation suggest that there was a specific level of participation at which the program began to pay off for experimentals; prior to reaching this level, participants experienced opportunity costs that exceeded their gains. In years three and four of the follow-up period combined, the nonexperimental analysis of impacts by amount of participation found a program effect of -$983 for early dropouts (fewer than 166 hours), $840 for those who received some JOBSTART services (between 166 and 533 hours), and $3,030 for those who substantially completed the program (more than 533 hours).[25] When compared to the four-year earnings impact of $814 for the full JOBSTART sample from the experimental analysis, it appears that those participants who spent many hours in the program reaped much greater than average rewards. The nonexperimental analysis also found that the impact for nonparticipants was very close to zero ($8), a finding that lends some support to the validity of the analysis since it corresponds to the expectation that nonparticipants in a voluntary program will have no impacts.

While the nonexperimental findings are consistent with the hypothesis that increasing service levels are correlated to larger four-year earnings impacts, their policy implications are unclear. First, the econometric methods used may not completely eliminate the effects of unobserved characteristics that could be linked to participation (such as motivation), and it is possible that these unobserved characteristics – rather than level of participation – are producing larger impacts. If this is true, working hard to keep everyone in the program longer would not necessarily raise average impacts.

This analytical problem aside, program operators could respond in different ways to the suggestive link between participation levels and impacts, although targeting recruitment efforts to those who are likely to participate intensively does not seem to be the answer, since in practice it is difficult to identify this group.[26] Further, such targeting may also result in a trade-off mentioned earlier: If young people who are likely to participate intensively are also likely to have good employment prospects without the help of the program, they will probably suffer large initial earnings losses as an opportunity cost of their participation in the program. There are, however, some clear lessons from the JOBSTART Demonstration about how the structure of the program affected participation. Youths in sequential/brokered JOBSTART programs (where education was provided in-house and the training by other agencies) had a much lower probability of making the transition to training and – as a result – averaged lower total hours of participation than youths in other types of sites.[27] This could have occurred for several reasons: Youths were deterred from going on to training by the prospect of applying to a new agency for admission; the training agencies did not see these youths as attractive participants; the youths' real interest was in the education component and they did not have an interest in training; or the transition to training occurred so late in the program that the youths were already beginning to "drift off" to other activities. Agencies considering operating sequential/brokered programs should consider the following suggestions to facilitate the transition to training:[28]

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[23] The relatively high participation rate, combined with an already modest impact, leads to a small correction that does not change the overall implications of the experimental JOBSTART findings. Also, because none of the nonparticipants received any JOBSTART services, benefit-cost estimates are unaffected by this correction.

[24] The nonexperimental analysis separated the experimental sample into four groups based on participation hours: those who did not participate in JOBSTART, and those in the bottom third (fewer than 166 hours), middle third (166 to 533 hours), and top third (more than 533 hours).

[25] Only this last figure is statistically significant.

[26] As program operators and researchers have learned through experience, it is hard to predict participation levels. Such factors as changes in housing arrangements, child care needs, and health problems can affect program participation; these issues are common among disadvantaged young people, but it is (continued...)

1. Develop agreements between service providers to give referrals from the education agency priority for admission to training.
2. Provide opportunities for participants to explore training options during the education phase to build their interest and enthusiasm.
3. Allow youths the option of an early transition to training so that occupational training is coupled with basic skills instruction.
4. Avoid gaps in service between the end of education activities and the beginning of training to facilitate a smooth transition.
5. Use paid work experience to keep youths in the program and to reinforce the relevance of skills learned in basic education to training and the world of work.
6. Streamline the application process as much as possible at the training agency.

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[26] (...continued)
difficult at program entry to know how different participants will be affected by them. The participation figures presented in Chapter 3 of this report illustrate that this was true in JOBSTART.

[27] As Chapter 3 reported, only 25 percent of experimentals participated in training in sequential/brokered sites, and they averaged only 307 hours, as opposed to 387 hours in concurrent sites and 519 hours in sequential/in-house sites. There were some problems in making the transition from education to training in sequential/in-house programs as well because of the longer time commitment required for these programs, but the transition problems were much more severe in sequential/brokered sites.

[28] These and other suggestions are discussed in more detail in Auspos et al., 1989, Chapter 10.

7. Designate a counselor/coordinator or case manager at the education agency to monitor and facilitate the progress of youths in the training phase.

D. Helping More Participants Receive a GED

As discussed in Chapter 4, one of JOBSTART's central objectives was to help participants obtain a high school diploma or GED certificate. It was shown that the program was quite successful in this regard: By the end of the second year of follow-up, 36.5 percent of experimentals had received a high school diploma or GED, compared to 21.3 percent of the control group, for a difference of 15.2 percentage points, significant at the one percent level.[29] However, the impacts on earnings were much more modest, raising questions about whether a GED makes little difference in earnings or whether it is valuable but JOBSTART helped too few people to get it.

Attempts to address these questions encounter analytical problems similar to those discussed in the previous section. Again, the basic difficulty in such a nonexperimental analysis is identifying the control group counterparts of experimentals who exhibit specific types of behavior after random assignment. In the forthcoming technical paper mentioned earlier, various techniques are used to understand the difference that attaining a GED through JOBSTART made for those who would otherwise not have done so.[30]

While the nonexperimental findings are again not definitive, a consistent pattern emerged that suggests that GED attainment was an important milestone in the JOBSTART program. The nonexperimental analysis included an effort to identify members of the experimental group who received a GED and their control group counterparts based on characteristics at random assignment, regardless of GED receipt during the four-year follow-up period. The earnings impact was especially large for the subgroup of these experimentals whose control group counterparts did not receive a GED during follow-up. This was in contrast to the small earnings impact for the subgroup whose control group counterparts had also received a GED. Thus, the overall JOBSTART earnings impact appears to be largely the result of substantial impacts for those youths identified through the nonexperimental analysis for whom access to JOBSTART was the key to receiving a GED.

These nonexperimental findings suggest that JOBSTART's earnings impacts may have been dependent on *who* was served. It appears that JOBSTART raised the earnings of those who needed the program to attain a GED; those who would have reached this milestone on their own or through other programs, or who would not have reached it in any case, showed small earnings impacts and do not appear to have benefited from their JOBSTART experience. In essence, JOBSTART's modest earnings impacts may well have resulted because GED impacts were not large enough. If this finding is correct, it has targeting implications similar to those discussed earlier in this chapter: Make sure that the program includes young people who are likely to be helped to get a GED through it, that is, those whose basic skills at program entry are neither so high that passing the GED is likely in any case, nor so low that the level of services provided would not be enough to help them pass. Although it may be difficult for program operators to identify this "middle" group with precision, basic skills testing at the time of application to the program would be helpful in targeting.

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[29] Most of this impact occurred through increased GED receipt rather than completion of high school.

[30] This nonexperimental analysis used the same techniques as the nonexperimental analysis of program participation; the control group counterparts were determined through various techniques based on pre-random assignment characteristics.

E. Strengthening Job Placement Assistance

Despite strong job placement assistance at CET/San Jose and a few other sites, it was generally the weakest component in the JOBSTART sites. In several sites, the JOBSTART participants were served by job placement staff who were also working with more job-ready clients. As a result, the understandably busy placement staff tended to work more with the non-JOBSTART clients, who tended to be easier to place. This experience highlights the importance of having staff – whether job placement staff or occupational trainers – who accept that helping less job-ready clients find employment is a central part of their job.

There were also job placement problems for youths who did not complete the JOBSTART services. This was especially apparent in sequential/brokered sites, where a large percentage of young people did not make the transition from education to training at another agency. In most programs, job placement services were focused on program graduates. It could be counterproductive to provide extensive job placement assistance to those in the early stages of program participation, because it might induce them to take a low-wage job when, with somewhat more participation, they could find a better job. However, it is important to have a "safety net" of placement assistance for those who stop participating before completing the program.

Finally, the JOBSTART experience illustrates the necessity of having job placement staff with good connections to employers and the ability to find training-related jobs with long-term prospects that are better than those the youths could find on their own. Job placement cannot be an afterthought, with responsibilities assigned to staff without the appropriate skills. The demands on job placement staff are as challenging as those facing education and training staff.

F. Continuing Services After the Initial Job Placement

Many young people served in JOBSTART found a job after leaving the program but fairly soon thereafter lost or left it. Like JOBSTART, many programs – especially those funded under JTPA – "terminate" a person at the time of initial job placement in order to claim a positive outcome for performance standards. Once terminated, a person cannot receive further services without reestablishing income eligibility. This practice seems ill-conceived for two reasons.

For many program participants, new problems and stresses emerge after they are placed in a job and start working. For example, young women with children discover that child care arrangements are less reliable than expected. Individuals receiving public assistance learn how tightly they may have to budget their resources when they start working and their welfare check is cut. Conflicts with fellow workers or supervisors may arise. At the very time when new and serious difficulties are appearing, support services are withdrawn.

Furthermore, this practice does not reflect the fact that few economically disadvantaged young people are able to make a major leap in economic status through a first job. More typically, the initial job is not particularly good, but it allows the youth to learn new skills on the job and build a work history and develop references, which can lead to a better job.

Some programs have sought to maintain a connection and provide counseling and other assistance as clients encounter difficulties in their first job or as they are ready to enter further training or find a new job. These programs hold out to participants an open-ended offer of assistance in making the many transitions needed to achieve self-sufficiency. They recognize that youth development is not a quick or straightforward process; young people try different options, move in and out of training and work, and encounter new problems with each new situation. As shown by the experiences of both the JOBSTART experimentals and controls, disadvantaged young people are not inactive. Most are involved in various education or training activities, or are working. The challenge is to help them build on each experience toward self-sufficiency.

While none of these operational suggestions comes with a guarantee, together they provide a strategy for building on the JOBSTART findings to improve the lives of economically disadvantaged young school dropouts.

APPENDICES

APPENDIX A

DATA SOURCES FOR THE EVALUATION

Many data sources were used in the evaluation of the JOBSTART Demonstration. Baseline demographic data were collected at the time of random assignment. Management information system (MIS) data from the sites were used to measure participation hours. Twelve-month, 24-month, and 48-month follow-up surveys of sample members were conducted to measure impacts on experimentals (including those who did not participate) compared to controls; the impacts concerned amounts of education and training received, employment and earnings, and other outcomes. The 12-month survey also dealt with the experiences of participants in the JOBSTART program. Much qualitative information, including interviews with program staff as well as focus groups and in-depth interviews with participants, was used in conjunction with the quantitative information. Each data source is described below.

I. JOBSTART Enrollment Forms

The JOBSTART enrollment form, designed by MDRC and filled out by program staff at the time of random assignment, was the major source of information about the demographic and socioeconomic characteristics of sample members. It included data on age, gender, ethnicity, family composition, educational attainment, and amount of time since dropping out of school, as well as basic information on welfare and employment histories. The enrollment form was completed for all but one sample member.[1]

II. JOBSTART Management Information System Forms

Sites used a number of MDRC-designed forms to report on the progress of participants in JOBSTART. The most important of these are described below.

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[1] This sample member was excluded from the impact analysis, since all demographic variables from the enrollment form were missing. For many of the sample members, a few specific pieces of demographic information were missing. In the impact analysis, the predicted values based on similar sample members were substituted for these missing observations.

A. Monthly Participation Report

The monthly participation report provided the number of hours that participants spent in basic education, occupational skills training, and other kinds of JOBSTART activities each month. It also provided information on the type of occupational skills training in which participants in training enrolled. Sites reported actual hours attended, not the number of hours scheduled.

Participation data used in this report were collected from August 1985 – the beginning of random assignment – through January 1989. The month of random assignment was included as a month of follow-up for participation, although the participant may have been randomly assigned late in the month. Those assigned in the last month of random assignment – November 1987 – had 15 months of follow-up participation data. The vast majority of the sample had at least 24 months of follow-up.

Collecting strictly comparable data across sites was not always possible for two reasons: First, the services provided in each site varied; second, there was some inconsistency in the way sites reported hours for activities other than basic education or occupational training classes. For example, a number of sites supplemented education and/or training classes with formal classroom instruction in a variety of topics generally termed "life skills." Some sites reported these as education hours; others counted them as training hours. In order to have similar definitions of the basic components – education and training – MDRC modified the reported hours at sites so that time spent in such activities as life skills classes was counted under "other activities."[2] The education hours reported by CET/San Jose also were adjusted to reflect only hours spent in the site's GED class.[3]

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[2] The sites affected were El Centro in Dallas, the Los Angeles Job Corps, the Atlanta Job Corps, and Allentown in Buffalo. At El Centro, one-half of all education hours prior to December 1986 were spent in life skills. After 1986, one-fourth of the reported education hours were spent in life skills. The hours were counted as hours in other activities by MDRC. At the Los Angeles Job Corps, participants spent one-half of their reported education hours in activities such as art, gym, and "world of work" for the first three months after enrollment. MDRC moved one-half of the education hours to hours in other activities for those months. At the Atlanta Job Corps, 10 hours each week were spent in activities such as life skills, driver education, and health. MDRC moved 28.6 percent of the reported education hours to hours in other activities. Allentown in Buffalo included such hours in its reported occupational training hours. MDRC moved all reported occupational training hours that did not have an associated type of training to hours in other activities.

[3] CET/San Jose reported 30 percent of each participant's occupational training hours as education, which included time spent on training-related basic skills in occupational training courses as well as hours in the site's GED class. For consistency with other sites, the education and training hours at CET/San Jose were recalculated by MDRC, and only hours spent in the separate GED class were included as education hours in this report.

Other differences remained, however. A number of sites offered limited amounts of work experience as part of the JOBSTART program. Some sites reported these hours as training hours; others reported them under other activities.[4] No adjustments were made to these hours. Finally, the Phoenix Job Corps did not report hours spent by participants in life skills or avocational activities, although the other two Job Corps sites did.

Appendix Table A.1 shows the common elements and variations in component activities across sites. In general, participation hours reported as being in the education component consisted of time spent in classes devoted to basic education or GED preparation; they did not include work on training-related basic skills done in occupational training courses. In all sites, participation hours that were counted in the training component included all activities offered in occupational training curricula, including units on training-related educational skills (such as Business English or Business Math) and employability development (instruction in work behaviors and job search). In the following sites, the hours counted as training also included time spent in work experience or on-the-job training: Connelley in Pittsburgh, El Centro in Dallas, the Phoenix Job Corps, and the Los Angeles Job Corps. Hours spent in other activities varied considerably across sites and included instruction in life skills, work experience, and orientation and avocational activities.

In order to assess the quality and completeness of the participation data, MDRC staff reviewed the teachers' class attendance records and other sources of data for a randomly selected sample of participants. For the most part, there was agreement between hours found in teachers' records and the monthly participation reports. If more than 20 percent of the cases in a quality control sample had discrepancies greater than 10 percent between site-reported hours and hours obtained in the check, MDRC scheduled either a recollection of the data or retraining of site staff, depending on the seriousness of the discrepancies.[5]

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[4] At EGOS in Denver, hours spent by participants in "work/study" were not reported.

[5] Because it was necessary to obtain records from a number of service providers, many of which did not maintain complete records for long periods, occupational training hours in brokered sites were the most difficult to confirm and probably have the greatest variation between actual and reported hours. The difficulty MDRC staff had in obtaining and verifying data from training providers reflects the difficulty sites had in monitoring hours for participants once they were no longer in the site. Problems were found even in the two sites with the best data from service providers: One site apparently overreported hours, while one site apparently underreported them. Because the number of participants who entered training in sequential/brokered sites was small, the misreporting of training hours did not greatly affect the average hours of training presented in the report.

TABLE A.1

ACTIVITIES INCLUDED IN PARTICIPATION HOURS,
BY SITE AND COMPONENT

Site	Education	Training	Other Activities
All sites	Classes in basic education or GED preparation	Classroom occupational skills training, including classes in training-related basic skills and employability development	Varied
Exceptions, by site			
Allentown (Buffalo)	—[a]	n/a	Life skills instruction included[a]
Atlanta Job Corps	—[a]	n/a	10-day orientation, work experience and on-the-job training, life skills and avocational activities included[a]
BSA (NYC)	A few hours per week in computer-assisted life skills instruction may be included	n/a	Life skills instruction included
CET/San Jose	—[a]	—[a]	No other activities
Chicago Commons	n/a	n/a	No other activities
Connelley (Pittsburgh)	n/a	Work experience mentorships included	No other activities[b]
CREC (Hartford)	Some hours in employability development activities included	n/a	Work experience internships included
East LA Skills Center	n/a	n/a	No other activities
EGOS (Denver)	n/a	—[c]	No other activites
El Centro (Dallas)	—[a]	Work experience internships included	Life skills instruction included[a]
LA Job Corps	—[a]	Work experience and on-the-job training included	5-day orientation, life skills and avocational activities included[a]
Phoenix Job Corps	n/a	Work experience and on-the-job training included	8-day orientation included[d]
SER/Corpus Christi	n/a	n/a	No other activities

SOURCE: Adapted from Auspos et al., 1989.

NOTES: [a] Reported hours were adjusted by MDRC.

[b] Site did not report participation in a one-hour afterschool component consisting of counseling and other support services in school year 1986-87.

[c] Site did not report participation hours in work/study positions.

[d] Site did not report participation hours in life skills and avocational activities.

B. Other Management Information System Data

As part of the monthly monitoring system, sites also reported on the end-of-month status of each participant; the participants who had been terminated and the reason for termination; and job placement and GED receipt among participants. The follow-up surveys proved to be a more complete source of data for employment and GED receipt, since they included activity by experimentals that might not have been reported to site operators as well as the experiences of the control group. Consequently, the surveys were the only source of these data used in this report.

III. Test of Adult Basic Education

The Test of Adult Basic Education (TABE), a modification of the California Achievement Test, was used to measure reading levels of experimentals. Prior research has shown the test to be a reliable and valid measure of reading ability. The test was used at two points in time: shortly after random assignment (as a baseline measure)[6] and after participants had spent some time in the program (usually after about 100 hours of education), as a measure of reading-level gains.[7]

About 20 percent of the total experimental sample did not take a baseline TABE. The percentage tested varied by site from a high of 100 percent to a low of 42 percent. The Job Corps sites and CET/San Jose had the lowest percentage of experimentals with baseline TABEs.

IV. Follow-Up Surveys

Eighty-four percent (1,941) of the 2,311 sample members randomly assigned to the experimental or control group between August 1985 and November 1987 were interviewed four years after random assignment. (Table A.2 presents 48-month survey response rates by site.) These 1,941 youths constituted the sample for this report, and each of them provided follow-up

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[6] In five sites, the TABE was also used as a test of reading-level eligibility and consequently was administered to controls as well as to experimentals. A number of other reading tests were administered in the other sites. Data from these sites were not included in the analysis because they were not comparable across sites. Scores on the eligibility test were used as the baseline measure for experimentals in sites where the TABE was used.

[7] The actual number of hours of education between random assignment and the first follow-up test varied considerably because of differences in measuring hours of education and delays in administering the tests. Also, in the first few months of the demonstration, sites were asked to test every three months, which resulted in considerable variation in the number of hours after which participants were tested.

TABLE A.2

RESPONSE RATES FOR THE 48-MONTH SURVEY, BY SITE

Site	Sample Size	Responded (%)	Did Not Respond (%)
Concurrent			
Atlanta Job Corps	80	86.3	13.8
CET/San Jose	200	83.5	16.5
Chicago Commons	93	80.7	19.4
Connelley (Pittsburgh)	219	84.0	16.0
East LA Skills Center	126	84.1	15.9
EGOS (Denver)	237	83.5	16.5
Phoenix Job Corps	153	87.6	12.4
SER/Corpus Christi	300	82.3	17.7
Sequential/in-house			
El Centro (Dallas)	200	89.5	10.5
LA Job Corps	296	78.0	22.0
Sequential/brokered			
Allentown (Buffalo)	147	91.8	8.2
BSA (NYC)	151	77.5	22.5
CREC (Hartford)	109	90.8	9.2

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 2,311 sample members randomly assigned in JOBSTART.

Rows may not total 100.0 percent because of rounding.

information for 48 months after the date of his or her random assignment. Most responded to the 12-, 24-, and 48-month follow-up surveys, while the remainder responded to special combination surveys covering the entire period since their last response. The surveys were conducted[8] either in person or, for those who had moved out of the area, by telephone, one, two, and four years after random assignment. The interviews lasted about 45 minutes and provided information about the sample member's experiences during the period of follow-up covered in that survey wave. Respondents were asked about their employment history, family status, welfare receipt, and receipt of education or training outside of JOBSTART. For the 12-month survey, experimentals who did not participate in JOBSTART were asked why; participants were asked what they liked and disliked about the program and their reasons for leaving. (Appendix B discusses issues of sample bias and data quality for the surveys.)

Sample members who could be located were generally willing to be interviewed. Some could not be located while others simply could not be contacted.

Some completed surveys lacked some information that was important in calculating impacts. Because the presence of missing data might have been correlated with an observed or unobserved prior attribute, dropping cases with missing data from the analysis might have biased the impact estimates or produced month-to-month inconsistencies. Imputing values is possible using a procedure that does not bias results. A separate regression was run for each variable with missing values, yielding predicted values for the missing data. These predicted values were used as estimates of the missing values. Continuous outcomes may contain outliers – extreme values that overly influence estimates. In the analysis, these were treated as missing, and the usual procedures for missing values were applied.

V. Qualitative Data

Qualitative descriptions of the program and participants' experiences in it were obtained from a variety of sources and were used to complement the analysis of the quantitative data.

MDRC research staff visited sites and conducted structured interviews with program administrators, counselor/coordinators, and teaching staff to determine recruitment practices; the content of services in the education and training components, job placement, and other activities;

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[8] MDRC contracted with the survey division of Abt Associates Inc., a Boston-based research firm, to implement, manage, and monitor the survey. Completed surveys were data-entered and checked for completeness by Abt. Members of the Abt staff also assisted in the design of the survey instrument.

the range of support services and retention strategies; and staffing patterns and staff experience with JOBSTART. MDRC staff also observed education and training classes in each site and visited some of the organizations that provided occupational training to JOBSTART participants in the sequential/brokered sites. Sites were typically visited by research staff once during the early phase of the demonstration and twice in the second year of program operations. This information was supplemented by ongoing reports on program operations and classroom observations provided by MDRC operations staff, who visited each site at regular intervals: once every month in year one and once every two months in year two of the operational period. (Interviews and observations concerning the education component were developed in conjunction with an education expert who worked with MDRC as a consultant.)

Information about participants' reactions to JOBSTART was obtained from focus group discussions with 46 JOBSTART participants in four sites between May 1987 and February 1988. Female participants were interviewed at Connelley in Pittsburgh and at BSA in New York City; men were interviewed at El Centro in Dallas and at the Los Angeles Job Corps. Each session was attended by between 9 and 14 participants and lasted between two and two and a half hours. At Connelley and El Centro, the groups were made up of participants in attendance on the session day; at the Los Angeles Job Corps, staff selected students who were doing well in the program; the BSA group included both current participants in education and women who had already moved on to occupational skills training. Because they included many participants who stayed longer than average and/or were doing well in the program, the groups were not representative of all JOBSTART participants. Nevertheless, used in conjunction with the survey responses, the focus group discussions provided valuable insights into participants' expectations about the program, what helped and hindered their participation, their opinions of the education and training components, and their recommendations for improving the program. MDRC hired consultants to develop the discussion topics, moderate the groups, and analyze the responses.

A series of in-depth interviews was conducted by another consultant with 15 JOBSTART participants in four other sites (CREC in Hartford, EGOS in Denver, Allentown in Buffalo, and the Atlanta Job Corps) between November 1986 and September 1987. These profiles provided additional, although impressionistic, information about the lives of some JOBSTART participants prior to and during the demonstration. The report also drew on the observations of JOBSTART staff and selected participants who attended a conference on youth employment initiatives, sponsored by MDRC, in October 1987.[9]

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[9] See Manpower Demonstration Research Corporation, 1988, for a summary of the conference discussions.

APPENDIX B

THE JOBSTART IMPACT ANALYSIS: METHODOLOGICAL ISSUES

As outlined in Chapter 2, several methodological issues had to be addressed to answer the key evaluation questions with accuracy.

I. Selection Bias

Did random assignment succeed in creating a group of JOBSTART controls with the same pre-program characteristics as JOBSTART experimentals? If sample members become experimentals or controls completely at random, there are no systematic measured or unmeasured differences between the two groups before program treatment. Under those circumstances, average outcomes among controls measure what average outcomes would have been among experimentals had the treatment not been available to them, and the difference in average outcomes between experimentals and controls measures the program's effect. If there are systematic preexisting differences between experimentals and controls, then measured differences in post-treatment outcomes confound true program effects with biases due to the selection of more people from some groups to be experimentals and more people from other groups to be controls.

Table B.1 presents, one at a time, average characteristics for experimentals, controls, and both groups together (the full 48-month impact sample). There were only slight differences between groups in a few characteristics, and no overall pattern of systematic differences between groups.

An alternative, more rigorous way to deal with this issue is to use linear regression analysis. To implement statistical tests for systematic experimental-control differences in those characteristics used in impact regressions, Table B.2 presents linear regression results measuring the extent of selection bias for the 1,941 members of the JOBSTART sample for whom there are 48 months of follow-up survey data. The "full sample" column of Table B.2 shows the same slight differences in individual characteristics and the same absence of systematic differences as Table B.1. The final entry in the column, the p-value of the F-statistic, is very close to one, providing strong evidence that there was no overall pattern of differences between experimentals and controls. It shows that random assignment created two groups without systematic overall

TABLE B.1

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY RESEARCH STATUS

Characteristic and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Full Sample (%)	p[a]
Gender					
Women	1,041	54.7	52.6	53.6	0.357
Men	900	45.3	47.4	46.4	
Ethnicity					
White, non-Hispanic	172	8.3	9.4	8.9	0.499
Black, non-Hispanic	860	44.5	44.1	44.3	
Hispanic	847	44.4	42.8	43.6	
Other	62	2.7	3.7	3.2	
Ethnicity, by gender					
Women					
White, non-Hispanic	97	5.3	4.7	5.0	0.407
Black, non-Hispanic	467	24.8	23.3	24.1	
Hispanic	451	23.3	23.2	23.2	
Other	26	1.3	1.4	1.3	
Men					
White, non-Hispanic	75	3.0	4.7	3.9	
Black, non-Hispanic	393	19.7	20.8	20.2	
Hispanic	396	21.2	19.6	20.4	
Other	36	1.4	2.3	1.9	
Parental status					
Women living with own child(ren)					
No	533	28.6	26.2	27.5	0.207
Yes	508	26.0	26.3	26.2	
Men who have own child(ren)					
No	785	38.7	42.3	40.4	
Yes	115	6.7	5.1	5.9	
Employed within past year					
No	914	47.2	47.0	47.1	0.945
Yes	1,027	52.8	53.0	52.9	
Prior employment, by gender					
Women employed within past year					
No	583	29.7	30.4	30.0	0.317
Yes	458	25.0	22.1	23.6	
Men employed within past year					
No	331	17.5	16.6	17.1	
Yes	569	27.8	30.8	29.3	
Sample size	1,941	988	953		

(continued)

TABLE B.1 (continued)

Characteristic and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Full Sample (%)	p[a]
Left school in grade 11 or 12					
No	1,140	57.7	59.8	58.7	0.343
Yes	801	42.3	40.2	41.3	
Received occupational training within past year					
No	1,615	84.3	82.1	83.2	0.184
Yes	326	15.7	17.9	16.8	
Age					
16-19	1,425	73.3	73.6	73.4	0.890
20 or 21	516	26.7	26.4	26.6	
Marital status					
Ever married	184	9.8	9.1	9.5	0.605
Never married	1,757	90.2	90.9	90.5	
Living in own household or with boy/girlfriend					
No	1,575	81.8	80.5	81.1	0.465
Yes	366	18.2	19.5	18.9	
Own AFDC case or receiving General Assistance					
No	1,418	74.5	71.6	73.1	0.146
Yes	523	25.5	28.4	26.9	
Own AFDC case					
No	1,522	79.8	77.0	78.4	0.143
Yes	419	20.2	23.0	21.6	
Receiving Food Stamps					
No	1,214	63.1	62.0	62.5	0.636
Yes	727	36.9	38.0	37.5	
Arrested since age 16					
No	1,649	84.7	85.2	85.0	0.764
Yes	292	15.3	14.8	15.0	
Lived with both parents at age 14					
No	1,264	66.5	63.7	65.1	0.195
Yes	677	33.5	36.3	34.9	
Sample size	1,941	988	953		

(continued)

TABLE B.1 (continued)

Characteristic and Subgroup	Sample Size	Experimentals (%)	Controls (%)	Full Sample (%)	p[a]
Site					
Concurrent					
Atlanta Job Corps	69	3.3	3.8	3.6	1.000
CET/San Jose	167	8.5	8.7	8.6	
Chicago Commons	75	4.1	3.7	3.9	
Connelley (Pittsburgh)	184	9.2	9.8	9.5	
East LA Skills Center	106	5.1	5.9	5.5	
EGOS (Denver)	198	10.4	10.0	10.2	
Phoenix Job Corps	134	7.1	6.7	6.9	
SER/Corpus Christi	247	12.7	12.8	12.7	
Sequential/In-house					
El Centro (Dallas)	179	9.4	9.0	9.2	
LA Job Corps	231	11.7	12.1	11.9	
Sequential/brokered					
Allentown (Buffalo)	135	7.2	6.7	7.0	
BSA (NYC)	117	6.1	6.0	6.0	
CREC (Hartford)	99	5.3	4.9	5.1	
Sample size	1,941	988	953		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,941 sample members for whom there were 48 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference in distributions of characteristics between groups: that is, p is the probability that observed proportions in each subgroup differ by research status only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE B.2

ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF ASSIGNMENT TO THE EXPERIMENTAL GROUP

Regressor or Statistic	Full Sample	Men	Custodial Mothers	All Other Women
Constant	0.509***	0.498***	0.506***	0.531***
	(0.011)	(0.017)	(0.022)	(0.022)
Site				
Connelley (Pittsburgh)	-0.008	-0.057	0.075	0.020
	(0.059)	(0.085)	(0.115)	(0.127)
CET/San Jose	-0.125	-0.058	0.108	0.005
	(0.054)	(0.074)	(0.190)	(0.104)
SER/Corpus Christi	—	—	—	—
EGOS (Denver)	0.014	-0.015	0.136	0.003
	(0.050)	(0.076)	(0.094)	(0.103)
Chicago Commons	0.028	-0.022	0.216	-0.087
	(0.072)	(0.100)	(0.156)	(0.155)
El Centro (Dallas)	0.014	0.018	-0.078	0.119
	(0.055)	(0.080)	(0.105)	(0.118)
BSA (NYC)	0.016	-0.052	0.100	0.026
	(0.061)	(0.087)	(0.134)	(0.125)
Allentown (Buffalo)	0.041	-0.098	0.139	0.161
	(0.062)	(0.095)	(0.117)	(0.129)
CREC (Hartford)	0.019	0.011	0.028	0.047
	(0.064)	(0.100)	(0.125)	(0.122)
Phoenix Job Corps	0.019	-0.103	0.028	0.211*
	(0.057)	(0.082)	(0.116)	(0.118)
East LA Skills Center	-0.021	-0.062	0.218	-0.076
	(0.060)	(0.080)	(0.159)	(0.120)
LA Job Corps	-0.006	-0.099	0.121	0.036
	(0.052)	(0.077)	(0.103)	(0.105)
Atlanta Job Corps	-0.026	0.017	-0.060	0.052
	(0.075)	(0.115)	(0.138)	(0.155)
Male	-0.045	—	—	—
	(0.030)			

(continued)

TABLE B.2 (continued)

Regressor or Statistic	Full Sample	Men	Custodial Mothers	All Other Women
White, non-Hispanic	-0.027	-0.063	0.062	-0.032
	(0.046)	(0.070)	(0.094)	(0.085)
Hispanic	0.024	0.037	0.014	-0.008
	(0.033)	(0.053)	(0.064)	(0.065)
Other ethnicity	-0.057	-0.076	-0.281	-0.024
	(0.075)	(0.107)	(0.296)	(0.130)
Age 20 or 21	0.009	0.019	0.013	-0.052
	(0.028)	(0.041)	(0.051)	(0.063)
No phone number on enrollment form	-0.052	-0.085	0.061	-0.131
	(0.055)	(0.089)	(0.097)	(0.104)
Male parent	0.075	0.060	---	---
	(0.052)	(0.056)		
Female parent living with own child(ren)	0.001	---	---	---
	(0.037)			
Limited English	0.013	0.013	-0.090	0.069
	(0.064)	(0.098)	(0.207)	(0.100)
Arrested since age 16	0.048	0.073	-0.078	0.018
	(0.041)	(0.047)	(0.142)	(0.106)
Convicted since age 16	-0.078	-0.058	-0.170	-0.241
	(0.061)	(0.068)	(0.229)	(0.189)
Own AFDC case	-0.028	-0.035	-0.035	-0.008
	(0.039)	(0.076)	(0.061)	(0.081)
Receiving Food Stamps	-0.024	-0.076	0.005	-0.053
	(0.033)	(0.051)	(0.057)	(0.070)
Never married	-0.010	-0.110	0.008	0.088
	(0.043)	(0.078)	(0.068)	(0.100)
Household AFDC case	0.075**	0.138**	0.134*	-0.054
	(0.036)	(0.055)	(0.076)	(0.068)
Receiving Medicaid	-0.028	0.041	-0.103*	0.036
	(0.034)	(0.054)	(0.061)	(0.073)
Left school in grade 11 or 12	0.027	0.030	0.022	0.029
	(0.025)	(0.036)	(0.051)	(0.049)

(continued)

TABLE B.2 (continued)

Regressor or Statistic	Full Sample	Men	Custodial Mothers	All Other Women
Lived with both parents at age 14	-0.034 (0.026)	0.005 (0.037)	-0.044 (0.054)	-0.064 (0.049)
Employed within past year	-0.006 (0.025)	-0.073 (0.038)	0.035 (0.050)	0.040 (0.047)
Number of observations	1,941	900	508	533
Number of experimentals	988	448	257	283
Number of controls	953	452	251	250
Degrees of freedom for error	1,909	870	479	504
Error mean square	0.251	0.249	0.252	0.254
R square	0.011	0.036	0.049	0.037
Mean of dependent variable	0.509	0.498	0.506	0.531
F-statistic	0.707	1.108	0.871	0.687
P-value of F-statistic	0.884	0.318	0.658	0.887

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: The dependent variable in each regression equation was unity for each experimental and zero for each control. Each characteristic on the right-hand side of each equation was measured as a deviation from its mean. The standard error of each coefficient estimate is enclosed in parentheses.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The p-value of the F-statistic is the probability of obtaining these coefficient estimates if the true chance of becoming an experimental did not vary with any characteristic. Thus, the closer the p-value is to unity, the more successful was random assignment in equating average characteristics of experimentals and controls.

differences in characteristics before enrollment. There was a statistically significant difference in only one individual characteristic for the full sample: Experimentals were more likely to live in a household with someone else who received AFDC.

The procedure used to calculate all the impacts for the full sample presented in this report took this slight difference in characteristics into account, and estimated the impacts that would have occurred had it not existed. A similar approach was used for the three key subgroups listed in the remaining columns of Table B.2 when calculating their impacts.

II. Nonresponse Bias

Were those sample members for whom there are continuous data for 48 months representative of the full JOBSTART sample, including survey nonresponders? A high degree of mobility among disadvantaged young dropouts makes it difficult for survey interviewers to locate all of them a year or two after they have been enrolled into a research sample. As noted in Appendix A, 1,941 of the 2,311 full-sample members furnished data covering 48 months, either at the 12-month, 24-month, and 48-month junctures, or at the 48-month juncture, for an overall response rate of 84 percent (85 percent for experimentals and 83 percent for controls).[1] See Table A.2 for site-specific information on response rates.

There were systematic differences in characteristics between those who responded to the surveys and those who did not respond. Table B.3 presents linear regression results measuring the extent to which average characteristics for the 1,941 survey responders differed from average characteristics at random assignment for the 370 nonresponders. Since the final entry in the "full sample" column, the p-value of the F-statistic, is zero to three decimal places, there is strong evidence of systematic differences between responders and nonresponders. Responders were significantly less likely to be male, and significantly more likely to be white or Hispanic, to be age 20 or 21, to have left school in grade 11 or 12, and to have lived with both parents at age 14. Better response was found at El Centro in Dallas, Allentown in Buffalo, and CREC in Hartford, even after taking differences in individual characteristics into account. Importantly, responders were not significantly more likely to be experimentals than controls.

When nonresponse is randomly distributed among members of both treatment and control

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[1] There are two types of nonresponse. Unit nonresponse is the failure to ascertain answers to any of the questionnaire items. Item nonresponse is the failure to obtain only some answers. All the response rates discussed here are unit response rates.

TABLE B.3

ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF UNIT SURVEY RESPONSE

Regressor or Statistic	Full Sample 1,941/2,311	Men 900/1,130	Custodial Mothers 508/578	All Other Women 533/603
Constant	0.840*** (0.008)	0.796*** (0.012)	0.879*** (0.013)	0.884*** (0.013)
Experimental status	0.017 (0.015)	0.033 (0.024)	-0.010 (0.027)	0.027 (0.026)
Site				
Connelley (Pittsburgh)	0.039 (0.039)	0.096 (0.061)	-0.047 (0.066)	0.056 (0.077)
CET/San Jose	0.007 (0.035)	0.027 (0.052)	-0.130 (0.104)	0.002 (0.063)
SER/Corpus Christi	---	---	---	---
EGOS (Denver)	0.001 (0.033)	-0.004 (0.053)	0.006 (0.056)	-0.029 (0.062)
Chicago Commons	-0.004 (0.047)	0.049 (0.069)	-0.039 (0.091)	-0.056 (0.092)
El Centro (Dallas)	0.106*** (0.036)	0.147** (0.057)	0.053 (0.063)	0.073 (0.072)
BSA (NYC)	-0.025 (0.039)	0.007 (0.060)	0.121 (0.083)	-0.123* (0.072)
Allentown (Buffalo)	0.108*** (0.042)	0.200** (0.069)	0.090 (0.070)	-0.013 (0.076)
CREC (Hartford)	0.096** (0.043)	0.102 (0.072)	0.093 (0.076)	0.088 (0.074)
Phoenix Job Corps	0.050 (0.038)	0.061 (0.059)	0.092 (0.070)	-0.004 (0.072)
East LA Skills Center	0.018 (0.040)	0.106* (0.059)	-0.113 (0.090)	-0.079 (0.071)
LA Job Corps	-0.033 (0.034)	0.032 (0.054)	-0.081 (0.061)	-0.076 (0.063)
Atlanta Job Corps	0.068 (0.050)	0.106 (0.083)	0.038 (0.083)	0.007 (0.091)
Male	-0.090*** (0.020)	---	---	---

(continued)

TABLE B.3 (continued)

Regressor or Statistic	Full Sample 1,941/2,311	Men 900/1,130	Custodial Mothers 508/578	All Other Women 533/603
White, non-Hispanic	0.058* (0.031)	0.036 (0.049)	0.105* (0.059)	0.087* (0.051)
Hispanic	0.050** (0.022)	0.052 (0.037)	0.021 (0.038)	0.070* (0.037)
Other ethnicity	0.072 (0.049)	0.094 (0.076)	-0.157 (0.148)	0.088 (0.075)
Age 20 or 21	0.044** (0.019)	0.038 (0.029)	0.055* (0.030)	0.059 (0.038)
No phone number on enrollment form	-0.024 (0.036)	-0.030 (0.063)	0.050 (0.061)	-0.098* (0.059)
Male parent	0.023 (0.034)	0.001 (0.040)	---	---
Female parent living with own child(ren)	-0.022 (0.025)	---	---	---
Limited English	-0.060 (0.041)	-0.041 (0.068)	-0.231** (0.099)	-0.038 (0.058)
Arrested since age 16	0.005 (0.027)	0.019 (0.034)	0.057 (0.082)	-0.084 (0.062)
Convicted since age 16	-0.061 (0.039)	-0.071 (0.048)	-0.024 (0.130)	-0.052 (0.107)
Own AFDC case	0.026 (0.026)	0.063 (0.057)	0.017 (0.037)	0.012 (0.048)
Receiving Food Stamps	-0.005 (0.022)	0.036 (0.037)	-0.052 (0.034)	-0.016 (0.041)
Never married	0.017 (0.029)	-0.034 (0.057)	0.029 (0.040)	0.030 (0.058)
Household AFDC case	0.016 (0.024)	-0.018 (0.039)	-0.014 (0.045)	0.076* (0.040)
Receiving Medicaid	0.017 (0.022)	0.009 (0.039)	0.022 (0.036)	0.000 (0.043)
Left school in grade 11 or 12	0.036** (0.016)	0.066 (0.026)	0.028 (0.030)	0.032 (0.029)

(continued)

TABLE B.3 (continued)

Regressor or Statistic	Full Sample 1,941/2,311	Men 900/1,130	Custodial Mothers 508/578	All Other Women 533/603
Lived with both parents at age 14	0.047***	0.053**	0.052	0.030
	(0.017)	(0.026)	(0.032)	(0.030)
Employed within past year	0.017	0.044*	0.010	-0.013
	(0.016)	(0.027)	(0.030)	(0.028)
Number of observations	2,311	1,130	578	603
Number of experimentals	1,162	551	296	315
Number of controls	1,149	579	282	288
Degrees of freedom for error	2,278	1,099	548	573
Error mean square	0.131	0.161	0.102	0.102
R square	0.042	0.010	0.092	0.058
Mean of dependent variable	0.840	0.796	0.879	0.884
F-statistic	3.103	1.394	1.919	1.218
P-value of F-statistic	0.000	0.078	0.003	0.202

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: The dependent variable in each regression equation was unity for survey response and zero otherwise. Each characteristic on the right-hand side of each equation was measured as a deviation from its mean. The standard error of each coefficient estimate is enclosed in parentheses.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The p-value of the F-statistic is the probability of obtaining these coefficient estimates if the true chance of responding to the survey did not vary with any characteristic. Thus, the closer the p-value is to zero, the more important are differences in characteristics between survey responders and nonresponders.

groups, it is troublesome only because it reduces the sample size and thus the statistical power to find impacts of a given magnitude. Randomly distributed nonresponse does not alter the expected values of adjusted mean outcomes, and thus does not bias impacts. However, when nonresponse is greater among one research group (which is not the case for this sample) or among members of either research group with certain characteristics (such as men), impacts may be biased slightly unless corrected for nonresponse. The most flexible correction for nonresponse is incorporation of an additional equation for survey response into a two-equation system with the impact equation. The success of attempts to implement such corrections is data-dependent, and the differential response rates found do not seem quite large enough to warrant such uncertain measures.

III. Impact of Participation Versus Impact of Assignment

Because the target population for the JOBSTART Demonstration consisted of young people who had histories of dropping out of education programs, it was difficult to get those selected for the program to attend and to retain attendees for substantial periods of time. However, everyone assigned to experimental status was included when calculating average impacts of JOBSTART. Therefore, impacts do not measure the impacts of participation in JOBSTART, but rather of assignment to the group eligible to receive JOBSTART services.[2] Thus, impact estimates average net outcomes for all experimentals, including nonparticipants. Nonparticipation "waters down" the program effect the experiment seeks to detect. Fortunately, only 111 of the 988 experimentals in the 48-month impact sample never participated in the program. Such low nonparticipation may have been due in part to successful negotiation with sites to place the point of random assignment after initial assessment but immediately before program services started.

When substantial nonparticipation occurs during an experimental evaluation of a program, techniques are available for calculating impacts of participation as well as impacts of assignment. When the proportion of assignees to the program who are not counted as participants is an unbiased measure of the proportion of controls who would not have participated, when the program has no effect on nonparticipants, and when the sample is large enough, it is approximately valid to use the formula[3]

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[2] Some might suggest that nonparticipants be excluded from impact analyses. However, such exclusions would expose impacts to possible selection biases, undermining the control group's validity in measuring what would have happened without the program. When nonparticipants are excluded from the experimental group, average measured and unmeasured characteristics of experimentals may no longer be the same as average control group characteristics. See Cave, 1988.

$$\text{Impact of participation} = \frac{\text{Impact of assignment}}{\text{Fraction participating}}.$$

Using this formula necessitates validating all of the assumptions underlying it, and thus makes impact analysis more complicated than a simple comparison of average outcomes for those assigned to the experimental group and those assigned to the control group. The assumption of zero effects on nonparticipants is troublesome, because the process of recruiting experimentals, screening them, and contacting them when they do not appear may alter their behavior. Thus, in this report, impacts of assignment were reported instead of impacts of participation.

As outlined above, impacts of assignment to JOBSTART were calculated by comparing average outcomes for all those assigned to the experimental group with average outcomes for all those assigned to the control group. In order to increase the statistical precision of the impact estimate, a variant of simple group averaging known as one-way linear analysis of covariance was used for the impact analysis in this report.[4] As shown for the 48-month sample of 1,941 survey responders in Table B.4, in a multiple regression of outcome on covariates measured at the time of enrollment and on a dummy variable for research status, the coefficient of the dummy variable is the impact. This coefficient may be interpreted as the difference between the adjusted mean outcome for those assigned to the experimental group and the adjusted mean outcome for those assigned to the control group. Adjustment removes the effect of slight differences at the time of enrollment in characteristics related to the outcome, and yields a purer measure of the effect of research status alone.

Some of the subgroup results presented in this report are based on slightly more complex regression equations, which include terms for interactions between experimental status and subgroup characteristics. Such "two-way ANCOVA" impacts may differ to some extent from "split-file" impacts estimated by eliminating other subgroups from "one-way ANCOVA" analyses for Table B.4. However, calculating two-way ANCOVA impacts easily permits determining the statistical significance of impact differences, and is less burdensome computationally.

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[3] See Cave, 1988; Auspos, Cave, and Long, 1988, Appendix E; Bloom, 1984; and Farkas et al., 1984, p. 85. If such an adjustment factor were appropriate here, its value would be approximately the reciprocal of the rate of participation in JOBSTART, or 1 / (1 - 111/988) = 1.127.

[4] See Cave, 1987, and Ostle, 1975.

TABLE B.4

ESTIMATED REGRESSION COEFFICIENTS FOR SELECTED OUTCOMES

	Dependent Variable			
Regressor or Statistic	Ever Received Any Education or Training, Months 1-48 (%)	Received GED or High School Diploma by End of Month 48 (%)	Ever Employed, Months 37-48 (%)	Total Earnings, Months 37-48 ($)
Constant	56.140*** (1.241)	28.570*** (1.498)	64.484*** (1.439)	5,182.009*** (190.042)
Experimental status	37.887*** (1.744)	13.407*** (2.106)	1.252 (2.023)	410.427 (267.115)
Site				
Connelley (Pittsburgh)	4.848 (4.484)	3.647 (5.414)	-14.483*** (5.200)	-567.502 (686.639)
CET/San Jose	-12.677*** (4.119)	-8.979* (4.972)	-6.827 (4.776)	3,314.370*** (630.623)
SER/Corpus Christi	---	---	---	---
EGOS (Denver)	3.800 (3.801)	-16.527*** (4.588)	2.452 (4.407)	1,436.536** (581.950)
Chicago Commons	2.828 (5.486)	-28.284*** (6.623)	0.072 (6.361)	1,178.044 (839.945)
El Centro (Dallas)	-5.761 (4.171)	5.567 (5.036)	5.567 (4.837)	1,393.053** (638.698)
BSA (NYC)	-0.368 (4.658)	-5.128 (5.624)	-15.119*** (5.402)	1,525.701** (713.296)
Allentown (Buffalo)	14.218*** (4.751)	-2.662 (5.736)	-17.592*** (5.510)	-667.935 (727.489)
CREC (Hartford)	-2.856 (4.846)	-16.671*** (5.850)	-29.055*** (5.619)	309.861 (741.975)
Phoenix Job Corps	-7.174* (4.318)	-10.467** (5.213)	-17.704*** (5.007)	-416.423 (661.121)
East LA Skills Center	10.109** (4.592)	-25.741*** (5.544)	-3.960 (5.325)	2,704.126*** (703.112)
LA Job Corps	-1.963 (3.953)	-12.121** (4.772)	-5.475 (4.584)	2,254.905*** (605.232)
Atlanta Job Corps	11.978** (5.740)	-7.344 (6.930)	-6.853 (6.656)	1,335.581 (878.901)
Male	-4.967** (2.258)	-1.646 (2.726)	17.290*** (2.618)	3,199.642*** (345.738)

(continued)

TABLE B.4 (continued)

	Dependent Variable			
Regressor or Statistic	Ever Received Any Education or Training, Months 1-48 (%)	Received GED or High School Diploma by End of Month 48 (%)	Ever Employed, Months 37-48 (%)	Total Earnings, Months 37-48 ($)
White, non-Hispanic	-4.325	7.786*	12.889***	1,984.234***
	(3.487)	(4.210)	(4.044)	(533.940)
Hispanic	-2.038	-2.996	8.241***	1,448.159***
	(2.552)	(3.081)	(2.960)	(390.789)
Other ethnicity	-1.405	-14.432**	12.375*	1,877.268**
	(5.735)	(6.924)	(6.651)	(878.160)
Age 20 or 21	0.706	-3.653	-3.634	-497.072
	(2.132)	(2.573)	(2.472)	(326.382)
No phone number on enrollment form	-5.939	-11.822**	-2.915	-568.788
	(4.168)	(5.031)	(4.833)	(638.142)
Male parent	-4.171	-5.019	6.856	1,003.750
	(4.005)	(4.836)	(4.645)	(613.302)
Female parent living with own child(ren)	-2.655	-1.640	-4.130	-6.260
	(2.847)	(3.436)	(3.301)	(435.855)
Limited English	-0.979	5.994	7.402	221.686
	(4.910)	(5.927)	(5.694)	(751.783)
Arrested since age 16	1.712	-6.288*	-1.956	-566.609
	(3.101)	(3.744)	(3.596)	(474.833)
Convicted since age 16	0.093	5.167	-10.607**	-1,931.240***
	(4.642)	(5.604)	(5.383)	(710.750)
Own AFDC case	5.868**	-4.920	-7.147**	-757.830*
	(2.970)	(3.505)	(3.444)	(454.750)
Receiving Food Stamps	-1.982	2.876	1.691	17.163
	(2.488)	(3.003)	(2.885)	(380.884)
Never married	-0.424	-3.139	-4.967	-332.938
	(3.279)	(3.958)	(3.802)	(502.059)
Household AFDC case	2.204	-5.643*	-4.544	-986.466**
	(2.756)	(3.327)	(3.196)	(421.994)
Receiving Medicaid	-4.347*	-0.387	-0.590	-114.056
	(2.581)	(3.116)	(2.993)	(395.156)
Left school in grade 11 or 12	-0.767	7.578***	3.319	992.220***
	(1.883)	(2.273)	(2.183)	(288.267)

(continued)

TABLE B.4 (continued)

	Dependent Variable			
Regressor or Statistic	Ever Received Any Education or Training, Months 1-48 (%)	Received GED or High School Diploma by End of Month 48 (%)	Ever Employed, Months 37-48 (%)	Total Earnings, Months 37-48 ($)
Lived with both parents at age 14	0.916	-1.258	3.358	321.694
	(1.946)	(2.349)	(2.257)	(297.984)
Employed within past year	1.184	4.356*	9.100***	1,572.63***
	(1.885)	(2.276)	(2.186)	(288.680)
Number of observations	1,941	1,941	1,941	2,311
Number of experimentals	988	988	988	1,162
Number of controls	953	953	953	1,149
Degrees of freedom for error	1,908	1,908	1,908	1,908
Error mean square	1,459.509	2,127.222	1,962.673	34,218,685.1
R square	0.226	0.086	0.151	0.188
Mean of dependent variable	75.425	35.394	65.121	5,390.923
F-statistic	17.408	5.578	10.571	13.815
P-value of F-statistic	0.000	0.000	0.000	0.000

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Ordinary least squares regression coefficients in this table correspond to impact estimates presented in Tables 4.1, 4.5, 5.1, and 5.4. A one-way linear analysis of covariance procedure was used to control for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; and Cave, 1987). The standard error of each coefficient estimate is enclosed in parentheses.

Each characteristic on the right-hand side of each equation was measured as a deviation from its mean.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

IV. The Internal Validity of Comparisons Among Subgroups and Types of Sites

Youths in the impact sample can be grouped based on their individual pre-random assignment characteristics or on the characteristics of the sites at which they applied for JOBSTART. Because such subgroup comparisons are a central part of the analysis presented in this report, it is important to discuss briefly the complications in drawing conclusions from any observed differences. Crucial comparisons of this type are between men and women, and between types of sites. To summarize, impacts can be compared across subgroups defined by individual or site characteristics, but more caution is advised in interpreting such results than in interpreting the full sample or within-site impacts described in the previous section. This is especially true for comparisons of site types.

The basic reason for such caution is that since sample members were not assigned randomly to these subgroups or types of sites, it may be impossible to attribute the difference in impacts to the single characteristic used to define the groups. For example, if women have bigger impacts than men, it may not be because they are women; the impact difference might really be because the women had less prior work experience, so controls were less likely to be working in the follow-up period. Further, using individual sites or site groupings for a subgroup impact comparison is fundamentally different from using an individual characteristic such as gender. Many things about sites differ (such as the local labor market, participant characteristics and interests, and program characteristics), and there is a real danger that impact differences for site groupings may be misinterpreted as measuring the relative efficiency of a single feature of a site's program, such as its curricula, facilities, or program structure (that is, brokered versus in-house services or concurrent versus sequential education and training), rather than other factors that could be driving inter-site variation.

The internal validity of impact comparisons by individual characteristics is difficult to test. However, there is a simple test for internal validity of impact comparisons by program features. If groups of individuals randomly assigned at two locations really differed *only* in the features of the programs that experimentals could attend, then the post-random assignment experience of controls at the two locations should be identical. This rarely happens; more typically, the experience of controls varies between sites, just as that of experimentals does.[5]

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[5] The usual situation is known in evaluation literature as "ecological correlation bias."

This problem can also affect impact comparisons for subgroups defined by characteristics of individuals as well as subgroup impacts by site or site grouping, although usually the concerns about misinterpretation are less severe. For example, if virtually all the Hispanics recruited into a demonstration are concentrated in one or two sites, then impacts for Hispanics are really site impacts. However, normally (and in the JOBSTART Demonstration, as discussed below) most measured characteristics of individuals are distributed fairly evenly across sites.[6] For example, there were younger and older sample members, people reading at higher and lower levels, and parents and childless youths in all the JOBSTART sites. Moreover, relevant unmeasured characteristics of individuals such as degree of motivation to attend a program and desire for a GED are likely to have been distributed fairly evenly among younger versus older sample members, those with higher versus lower reading levels, those who were parents versus those who were childless, and other subgroups defined by the observed characteristics of individuals.[7] For example, if the impact on educational attainment was higher among low-reading-level than among high-reading-level sample members, it is reasonable to interpret this as evidence that the programs were more effective with the former subgroup, rather than concluding that higher educational attainment occurred because the average sample member who had a low reading level was more motivated or wanted a GED more than the average sample member who had a higher reading level.

In contrast, when a sample is split by site or site group, unmeasured characteristics will be distributed unevenly across groups. For example, sites that were known for providing education services and that offered a sequence of basic skills instruction followed by occupational training at another agency (sequential/brokered sites) may have been more likely to recruit clients motivated to get a GED than were concurrent sites with a reputation for training, whose typical client may have wanted to learn occupational skills. Thus, a finding that JOBSTART's impact on GED attainment was smaller at concurrent sites than at sequential/brokered sites does not necessarily mean that someone with average motivation and desire for a GED has a better chance of getting a GED in a sequential program. Such a finding could mean that those recruited at sequential sites had, on average, very different levels of desire for skills training relative to GED preparation compared to those recruited at concurrent sites. Even if the usual statistical adjustment methods are employed in calculating impacts, little can be done about this problem, since motivation to participate in particular components was not measured.[8]

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[6] Several ethnic groups were concentrated in a few JOBSTART sites, however.

[7] To lessen this problem, impact estimates presented in this report always used site dummies as covariates when calculating impacts by individual characteristics; these dummies can correct for small differences between subgroups defined by individual characteristics in unmeasured characteristics associated with site.

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[8] Subgroup impact equations for groupings by site type cannot use individual site dummies to correct for small unobserved differences in groups in the same way that equations for groups defined by individual characteristics can: Individual site dummies would be highly correlated with the site groupings. To measure which delivery system is better for those with average levels of motivation, desire for GEDs, and other unobserved characteristics, the best approach is to randomly assign people to each delivery system in each location after carrying out a common recruitment effort at that location. In that way, unmeasured characteristics would be the same for each delivery system, because each delivery system would be fairly represented in each location and in each recruitment effort.

APPENDIX C

COST OF THE JOBSTART PROGRAM

I. The General Approach to Determining JOBSTART's Costs

This appendix describes the data sources and methodology used to estimate the cost of the JOBSTART program in each of the 13 demonstration sites.[1] It also discusses the factors contributing to the wide variation in costs across sites and examines the relative influence of different JOBSTART components on overall program costs.

The central objective of the analysis was to identify the market value of *all* resources used in providing JOBSTART services. It therefore counted as program costs not only the expenditures made by the agencies sponsoring the program, but also those made by outside organizations responsible for providing certain components (such as occupational skills training in the three sequential/brokered sites). Furthermore, in sites where goods and services that affected the nature of the program treatment were donated to the sponsoring agency, the analysis estimated the market value of those contributions and counted it, too, as a program cost.[2] For these reasons, the costs presented here may differ from those reflected in a sponsoring agency's own fiscal records.

This appendix does not present estimates of the cost of education and training services received by members of the control group. Thus, it provides no insights into the *incremental* investment that the JOBSTART sites made for the experimental group. An estimate of these incremental or "net" costs is incorporated in the benefit-cost analysis presented in Chapter 7. In that analysis, the value of experimental-control differences in earnings and other outcomes (the "benefits") is compared with the experimental-control differences in the cost of services producing

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This appendix was written by Barbara L. Fink.

[1] The cost estimates in this appendix were generated as part of the 24-month impact analysis (Cave and Doolittle, 1991) and consequently reflect participation during only the first two years of follow-up. However, since the experimental group received virtually no JOBSTART services after the first two years, no additional cost data were collected for this report. This decision is reflected in both this appendix and the benefit-cost analysis in Chapter 7.

[2] For example, at Chicago Commons, many of the basic supplies essential to operating some of the training courses were donated to the program. The estimated value of these supplies, as reported in the agency's annual audit report, was thus counted as a program cost. As another illustration, the life skills workshops at both Chicago Commons and Connelley in Pittsburgh were conducted free of charge at the program site by outside organizations. These donated services were thus valued and included in the total cost. Their estimated value was based on the number of sessions conducted and by a proxy value of the average cost per session to the agency providing the service.

those benefits. As Chapter 7 shows, there is virtually no difference between the net costs of JOBSTART and the gross costs reported here.

A. Data Sources and Accounting Periods

Data for the cost analysis were gathered from a variety of sources. These include:

- individual staff salary information;
- site expenditure reports, which showed overall expenditures on salaries and fringe benefits, rent, utilities, supplies, equipment, administration, and so on;
- program enrollment and participation data covering JOBSTART and non-JOBSTART participants, both for the program as a whole and for individual components (such as education classes and training classes);
- JTPA expenditure data in sites where JTPA funds were used to provide program services;
- agency data on support service expenditures covering needs-based payments, transportation, food, child care, and other participant payments;
- interviews with program staff concerning the allocation of staff time across program components and between JOBSTART and non-JOBSTART functions, and other aspects of site operations that affected the use of resources; and
- MDRC's MIS data on the experimental group's degree of participation in JOBSTART activities during the first 24 months of follow-up, as discussed above.

In most cases, data from these sources covered a one-year "steady-state" period sometime between 1985 and 1988 (depending on the site), the years during which JOBSTART was funded.[3] However, the actual calendar months of this accounting period varied according to each site's date of entry into the demonstration and the particular months covered by its annual fiscal reporting period.[4]

Ideally, a steady-state period should reflect a time during which program operations are relatively stable. Although it was difficult to define such a period for JOBSTART because of the demonstration's relatively short duration, the period selected in most sites began at least several months after the initiation of the project (in order to avoid the start-up costs associated with beginning a new program) and ended at least several months prior to the termination of the demonstration (in order to exclude the phasedown period).[5] To remove the influence of inflation resulting from the use of costs from different calendar periods in different sites, all estimates were inflated or deflated to 1986 dollars.

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[3] The JOBSTART program at Connelley in Pittsburgh and SER/Corpus Christi changed substantially from the first year of operations to the next. Consequently, cost and participation data for both years were used.

[4] In some sites, participation data and expenditure reports did not cover exactly the same time period, so a number of additional adjustments had to be made in estimating average steady-state expenditures.

B. Excluded Costs

In estimating the average cost per JOBSTART experimental, adjustments were made to exclude two categories of expenditures embedded in the sites' fiscal data: (1) research-related costs and (2) the costs of services or activities that were offered to or used by non-JOBSTART participants. A fraction of program expenditures during the steady-state period resulted exclusively from research requirements. These included the extra costs involved in recruiting and processing individuals who became part of the control group, as well as the costs of staff time spent on conducting random assignment, completing the research enrollment forms, and participating in research-related interviews with MDRC personnel. These activities were not part of the normal effort of operating a JOBSTART program. Thus, the resources spent on them were not counted in the average cost estimates reported here.

Several of the sites also offered a number of services that were not a part of the JOBSTART program but were nonetheless captured in the agencies' aggregate expenditure reports. These too were excluded from the estimates of JOBSTART costs. This issue was most significant in the three Job Corps sites, where some Corpsmembers lived in dormitories at the centers, while others lived at home while attending Job Corps activities. All of the JOBSTART experimentals were nonresidents in these sites and were thus unaffected by the services intended exclusively for the residents. These residential-only services included: dormitory provisions, most nighttime and weekend recreational activities,[6] and supervision by residential advisors and dormitory attendants. The share of total Job Corps costs associated with exclusively residential aspects of the program was thus estimated and eliminated from the resources counted in determining the cost of JOBSTART.[7]

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[5] Because the core education and training services in most sites were already in place prior to JOBSTART and continued after the demonstration, start-up and phasedown costs for the JOBSTART Demonstration were not an issue for these components.

[6] Although nonresidents were invited to participate in all recreation activities, they did so much less frequently than residential Corpsmembers.

[7] A technical assistance project at BSA in New York City is another example of separate activities whose costs were excluded in estimating JOBSTART costs.

C. Calculating the Average Cost per Experimental

In each site, the total average cost of JOBSTART per experimental was determined by summing the average cost of several relatively distinct program components and services. Determining these component costs involved several steps. First, an average unit cost during the steady-state period – that is, the cost of serving one person in the component for a specified unit of time – was calculated. The unit of measure varied for some components, mostly depending on whether the activity operated on an open-entry/open-exit or a fixed-cycle basis.[8] Thus, for open-entry components, the average cost of serving one person for one month in the activity was estimated; for fixed-cycle activities, the average cost per person who ever entered a given cycle of the activity was estimated.

The numerator in these unit costs incorporated total expenditures for personnel and overhead functions, including expenses incurred for non-JOBSTART participants in sites where the experimentals were enrolled along with other persons in regular agency activities.[9] The value of donated goods and services was also counted as a program expenditure. The denominator included all participants (both JOBSTART and non-JOBSTART) in the component. Thus, for example, the unit cost of basic education at a site is the full cost of classes in which JOBSTART participants were enrolled, divided by the total number of students in the classes.

The average unit cost was then multiplied by a corresponding participation measure.[10] For open-entry components, the unit cost was multiplied by the average number of months in which experimentals spent any hours in the activity (including zero months for experimentals with no hours in the activity).[11] For components that operated on a fixed-cycle basis, the unit cost (the cost per person who entered the activity) was multiplied by the proportion of JOBSTART experimentals who ever entered the activity.[12] The values for both types of participation measures were based on the experiences of the experimentals in the 24-month impact sample, not of all experimentals who were randomly assigned.[13]

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[8] When participants leave open-entry activities, they are typically replaced by other individuals. Thus, the average value of resources expended per person for these components varies with the length of time an average participant receives that service. However, if entry into a training class is based on a fixed cycle and a student who drops out is not replaced by another student, the costs for that student's "slot" are still incurred by the agency on that student's behalf, regardless of his or her length of stay.

[9] For some activities, sites mainstreamed JOBSTART participants with non-JOBSTART participants. Other activities (for example, counseling, life skills instruction, basic education, or training services that were not normally provided as part of the agency's program) included JOBSTART participants only. See Auspos et al., 1989, for more details on the adaptations the sites made for JOBSTART.

[10] In order to spread average unit costs among all experimentals and to cover the full period of their involvement in the program, the participation measures captured participation that occurred at any time during the demonstration, not just within the steady-state period.

[11] This approach (that is, multiplying the average cost of serving one person for one month by the average number of months that JOBSTART experimentals spent in the component) allocates costs between JOBSTART and non-JOBSTART participants on the basis of their respective lengths of stay in the activity.

II. Accounting for Site Variations in Average Costs

The average total cost of JOBSTART's core components per experimental varied widely across the sites. (See Table C.1.) Although it fell within $4,500 to $6,500 in most sites, it ranged from less than $2,100 in CET/San Jose, EGOS in Denver, and SER/Corpus Christi to a high of about $7,500 in BSA in New York City. Several factors account for this diversity. Most notably, the sites differed in terms of *both* the amount of experimentals' participation in JOBSTART and the unit cost of that participation. This can be seen in Table C.1, which presents information for each site on the average number of months that the experimental group participated in program activities and the average monthly cost of participation.[14] Sites where the value of both of these variables was lower than in other sites were among the least expensive JOBSTART programs. For example, this combination of factors helps to explain why CET/San Jose (where experimentals participated for only 4.4 months and the average monthly cost was only $462) had the least expensive JOBSTART program.

In several sites, higher unit costs were somewhat offset by shorter participation, yielding lower total average costs than were observed in some other sites. For example, the average monthly cost of JOBSTART at the Atlanta Job Corps was higher than at Connelley in Pittsburgh ($845 compared to $566). However, Atlanta's overall average cost was lower ($4,173 compared to $5,185) because its experimentals were active in the program for less time (4.94 months compared to 9.16 months).

The wide variation in average monthly costs across the sites (ranging from $303 in EGOS in Denver to $1,569 in BSA in New York City) has a number of sources. One is enrollment levels.

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[12] This approach assumes that there was no difference in the average cost of serving JOBSTART and non-JOBSTART participants who actually began the component. The costs were considered fixed, whether or not the students stayed until completion. No data were available for comparing the lengths of stay of JOBSTART and non-JOBSTART participants.

[13] See Cave and Doolittle, 1991, for a discussion of the 24-month impact sample. In general, the experimentals in this sample, on average, had slightly more months with any hours of participation than did all experimentals, and thus may have been slightly more expensive to serve than those not in this sample.

[14] The average monthly cost for each site – created to facilitate comparisons across sites – was calculated by dividing the average total cost (for experimentals) by the average number of months active in JOBSTART (for experimentals). Although this calculation assumes that the average total cost in all sites was variable, as noted earlier, the costs of fixed-cycle activities were actually calculated on a fixed-cycle basis.

TABLE C.1

AVERAGE MONTHLY AND TOTAL COSTS PER EXPERIMENTAL AND PER PARTICIPANT, BY SITE

Site	Experimentals with Any Hours of Participation (%)	Average Number of Months Active in JOBSTART[a]		Average Cost per Month Active in JOBSTART ($)	Average Total Cost for Core Components ($)	
		Experimentals	Participants		Experimentals	Participants[b]
Concurrent						
Atlanta Job Corps	84.80	4.94	5.82	845	4,173	4,921
CET/San Jose	64.00	4.40[c]	6.88[c]	462	2,034	3,178
Chicago Commons	91.90	4.32	4.71	1,499	6,477	7,048
Connelley (Pittsburgh)	98.90	9.16	9.26	566	5,185	5,243
East LA Skills Center	82.40	5.04	6.12	970	4,887	5,931
EGOS (Denver)	93.55	6.86[d]	7.33[d]	303	2,076	2,219
Phoenix Job Corps	86.57	6.25	7.22	793	4,956	5,725
SER/Corpus Christi	98.30	5.03	5.12	417	2,098	2,134
Sequential/in-house						
El Centro (Dallas)	100.00	5.25	5.25	1,011	5,306	5,306
LA Job Corps	79.10	7.17	9.05	774	5,550	7,016
Sequential/brokered						
Allentown (Buffalo)	100.00	8.88	8.88	660	5,862	5,862
BSA (NYC)	75.38	4.77	6.33	1,569	7,484	9,928
CREC (Hartford)	88.89	5.60	6.30	923	5,166	5,812

SOURCE: MDRC calculations from site and MDRC participation, fiscal, and administrative data.

NOTES: Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate. "Participants" are the subset of experimentals who were active for at least one hour in any JOBSTART component within 24 months of random assignment.

All costs are in 1986 dollars.

[a] Unless otherwise stated, the number of months active in JOBSTART is defined as the number of months with hours in any JOBSTART component.

[b] These estimates were obtained by dividing the average total cost per experimental by the percentage of experimentals with any hours of participation.

[c] For consistency with the definition of unit costs in this site, the number of months active in JOBSTART is measured from the month of random assignment to the last month with hours in any component.

[d] For consistency with the definition of unit costs in this site, the number of months active in JOBSTART is measured from the first month with hours in any JOBSTART component to the last month with hours in any component.

For example, if the number of participants "on board" a program in a typical month is high relative to the number of program instructors, the total monthly instructional costs (and the corresponding overhead expenditures) will be spread over many people, lowering the average unit cost per participant. This factor helps to account for the relatively low monthly cost of JOBSTART at EGOS in Denver, a large public vocational school with more than 15,000 students. In contrast, at BSA in New York City, high monthly costs were partly the result of its having enrolled only about half the number of students the school had the capacity to serve at any one time. Staffing decisions can also affect costs. For example, Chicago Commons assigned two instructors to all training classes, an unusual practice among the JOBSTART sites, and this raised its average monthly cost per participant. Differences in wage scales further explain some of the variation in monthly costs. As an illustration, the average hourly wage paid to instructors at SER/Corpus Christi was about half the hourly rate received by teachers at the East Los Angeles Skills Center.

Differences in the scope of activities and services across the sites also account for differences in average monthly costs. For instance, as will be seen below, the three least expensive sites had no life skills or work-readiness instruction, and one of them (CET/San Jose) spent little on support service payments and basic education as a separate activity. Differences in overhead costs, such as those for rent and administration, also varied across the JOBSTART sites.

Table C.1 shows that in some sites a substantial proportion of experimentals left the program after random assignment and so never entered any program component. At the East Los Angeles Skills Center, for example, only 82 percent of the experimental group ever received JOBSTART services. One consequence of such attrition is that a site's average cost *per person actually served* by the program is higher than its average cost *per experimental*.[15] At the Los Angeles Job Corps, it was 26 percent higher ($7,016 compared to $5,550). Although the average cost per experimental would be the appropriate number to include in a benefit-cost analysis for the JOBSTART evaluation, the average cost per participant may be a better guide for administrators interested in the implications for an agency's budget of operating a JOBSTART program.

III. Component Costs

This section discusses how the costs of the individual JOBSTART components contributed to the total average cost at each site and further illustrates the sources of variation in the total costs across the sites.

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[15] The average cost per participant was calculated by dividing the average cost per experimental by the percentage of experimentals with any hours of participation.

A. Definitions of Components

For purposes of the cost analysis, JOBSTART functions were divided into eight main components. The category of *recruitment, intake, and orientation* was defined to encompass sites' efforts to attract and enroll individuals into the JOBSTART program and to prepare them, through special presentations or workshops, for attending the agency's regular education and training classes. This process involved screening applicants to determine whether they met all JOBSTART as well as JTPA or Job Corps eligibility criteria.[16] Random assignment, special data collection, and the additional efforts devoted to recruiting and processing extra individuals to allow for the creation of a control group also occurred during the recruitment and intake stages. As previously mentioned, these latter activities were defined as research-only costs, and hence they were not counted in the average cost of this component.

Following orientation, experimentals in all sites were scheduled to attend *basic education* classes. *Occupational skills training* classes were offered concurrently or following the completion of basic education. The costs of these two components were estimated separately, although in some sites the line between them was not sharp.[17]

Several sites also enriched their programs by offering *work-readiness classes or life skills workshops* that covered topics such as work habits, health, and financial management. In addition, the Job Corps sites offered avocational classes in driver education, sewing, and physical education. These were counted as part of the work-readiness/life skills component for the cost analysis.

Job placement was defined to include instruction in job-seeking techniques as well as direct placement efforts. *Coordination and counseling* include staff efforts to monitor participants' attendance and progress in JOBSTART activities and to counsel them on an as-needed basis. In a number of sites where JOBSTART was operated alongside other programs, a special counselor was designated to perform this function exclusively for the experimental group.

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[16] The costs included here for JTPA eligibility determination only cover a site's efforts to help applicants identify and collect the necessary documents and complete the required paperwork as part of the application process. They generally do not include the time that JTPA staff spent reviewing those documents and approving the applications.

[17] Data limitations have precluded perfect consistency across all sites in the definition of each component. Especially problematic is the distinction between basic education and skills training at CET/San Jose and Chicago Commons. To a large extent, basic education instruction in these two sites was integrated with occupational skills training. However, these sites also operated separate remedial education classes. The cost analysis counted only participation in those remediation classes as basic education. This definition of basic education is consistent with that used in the calculation of education hours, as reported in Chapter 3.

Support services were defined as the special expenditures intended to help motivate participants to attend program activities regularly, or to help offset some of the potential barriers to attendance. The particular types of support services that were available varied across sites, but included payments for child care, transportation, needs-based payments, food, and attendance and achievement awards.

Medical and dental services were an additional component offered in the three Job Corps sites through an on-site clinic. To a much lesser extent, such services were also offered at EGOS in Denver through a formal agreement with an outside agency to which staff routinely referred participants.[18] These services are not considered to be part of the core JOBSTART model, however. Consequently, their costs have not been included in the total average costs reported above (although they have been estimated).

It should be noted that the information used in allocating total site costs across components (such as the proportion of staff time spent on recruitment and intake versus counseling and coordination) was often imprecise. As a result, some component cost estimates are much less certain than others. However, this does not affect the estimate of any site's average total cost.

B. Variations in the Cost of JOBSTART Components

Table C.2 shows the estimated average cost per experimental of each JOBSTART component for each site. In addition, for the four sites where medical and dental services were provided, the table shows how the total average cost changed when expenditures for these services were counted. In the three Job Corps sites, these amounted to fairly sizable expenditures – $564 per experimental at Los Angeles, $690 at Atlanta, and $357 at Phoenix. In addition, EGOS in Denver, which, through routine referrals, provided eyeglasses and dental examinations, spent $24 per experimental on medical expenses.

Recruitment, intake, and orientation activities accounted for between 9 and 13 percent of the total average cost of the core JOBSTART components in most of the sites, but reached as high as 19 percent at the East Los Angeles Skills Center. In absolute value, BSA in New York City spent the most on these upfront efforts ($1,313), while SER/Corpus Christi spent the least ($227 per experimental).

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[18] Some of the other JOBSTART sites also referred participants to such services at outside agencies, but on a much less formal basis. In these sites, the costs incurred by those agencies were not estimated.

TABLE C.2

AVERAGE JOBSTART OPERATING COSTS PER EXPERIMENTAL, BY COMPONENT AND SITE

	Concurrent							
Component	Atlanta Job Corps ($)	CET/ San Jose ($)	Chicago Commons ($)	Connelley (Pittsburgh) ($)	East LA Skills Center ($)	EGOS (Denver) ($)	Phoenix Job Corps ($)	SER/ Corpus Christi ($)
Recruitment, intake, and orientation	602	245	327	445	923	245	645	227
Basic education	529	88	1,400	644	1,114	384	939	632
Occupational skills training	283	1,031	2,931	793	1,531	297	1,446	533
Work-readiness or life skills training	839	n/a	35	283	n/a	n/a	343	n/a
Job development and placement assistance	92	308	262	334	36	19	188	73
Counseling and program coordination	1,031	301	1,159	2,239	1,196	664	498	633
Support services[a]	797	61	363	447	87	467	897	593[b]
Subtotal for core JOBSTART components	4,173	2,034	6,477	5,185	4,887	2,076	4,956	2,098
Medical/dental[c]	690	n/a	n/a	n/a	n/a	24	357	n/a
Total	4,863	2,034	6,477	5,185	4,887	2,100	5,313	2,098

(continued)

TABLE C.2 (continued)

Component	Sequential/ In-House		Sequential/ Brokered		
	El Centro (Dallas) ($)	LA Job Corps ($)	Allentown (Buffalo) ($)	BSA (NYC) ($)	CREC (Hartford) ($)
Recruitment, intake, and orientation	568	586	328	1,313	689
Basic education	1,301	648	1,147	3,836	2,634
Occupational skills training	1,175	1,478	529	453	332
Work-readiness or life skills training	392	442	1,438	920	n/a
Job development and placement assistance	639	302	628	n/a	n/a
Counseling and program coordination	719	705	1,058	757	1,279
Support services[a]	512	1,389	734	205	232
Subtotal for core JOBSTART components	5,306	5,550	5,862	7,484	5,166
Medical/dental[c]	n/a	564	n/a	n/a	n/a
Total	5,306	6,114	5,862	7,484	5,166

SOURCE: MDRC calculations from site and MDRC participation, fiscal, and administrative data.

NOTES: Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate.

All costs are in 1986 dollars.

[a] Includes such costs as needs-based and incentive payments; transportation, child care, and clothing allowances; and food.

[b] Because of data limitations, the support services cost for JOBSTART and non-JOBSTART participants in this site could not be separated from other expenditures in the general overhead rate used in estimating the cost of the other program components. Thus, the per-experimental cost of each component includes the cost for support services. To avoid double-counting this expenditure in the average total cost per experimental, the $593 estimated value of support services, which was calculated from individual-level data available only for JOBSTART youths, is not included in the sum of component costs.

[c] These services were routinely available in and measured for the Job Corps sites and EGOS only.

Basic education, skills training, and coordination and counseling were usually among the most expensive components to operate across the sites. When taken together, they accounted for no less than 44 percent (the estimate for the Atlanta Job Corps) of the total average cost of the core JOBSTART components, and they reached as high as 86 percent (the estimate for SER/Corpus Christi). In most of the sites, this combination of functions accounted for at least two-thirds of the total average cost.

Particularly notable are the high costs of basic education at the three sequential/brokered sites. At BSA in New York City and CREC in Hartford, basic education alone accounted for half the total cost ($3,836 and $2,634 per experimental, respectively). The cost of basic education was also high at Chicago Commons ($1,400), which added a separate education class specifically for JOBSTART participants, and at El Centro in Dallas ($1,301) and the East Los Angeles Skills Center ($1,114). Overall, the percentage of total average costs devoted strictly to basic education ranged from 4 percent at CET/San Jose to 51 percent at BSA and CREC. Education costs were especially low at CET/San Jose ($88 per experimental), in part because most of the hours that experimentals spent in that site were in training classes, which, it should be recalled, also included some work on basic education skills. (Basic education that occurred in the context of occupational skills training was counted as skills training.)

The resources spent on skills training also varied widely across the sites. Not surprisingly, as a proportion of total average costs, expenditures for this component were lowest at the sequential/brokered sites (accounting for less than 10 percent of those costs), where only about one-quarter of the experimentals made the transition to a training activity. In the other sites, this component accounted for between 7 and 51 percent of total costs. In absolute value, it was most expensive at Chicago Commons ($2,931), where the training classes were small and operated on a fixed-cycle basis (dropouts within a cycle were usually not replaced by other students). The classes also involved fairly technical instruction and used significant amounts of purchased and donated supplies.

Across all sites, the per-experimental cost of coordination and counseling ranged from $301 at CET/San Jose to $2,239 at Connelley in Pittsburgh, where it accounted for an unusually high 43 percent of the total average cost per experimental. In contrast, job search assistance and placement in most sites accounted for no more than 6 percent of total average costs, although it ranged from $19 per experimental at EGOS in Denver to $639 at El Centro in Dallas. In most sites, this tended to be a relatively inexpensive component because relatively few staff members were usually assigned to instruct or assist many different participants. (In two of the three sequential/brokered sites, job search assistance was not offered by the sponsoring agency but, instead, was a function expected to be performed by the outside training vendors.)

The sites also varied widely in their expenditures on work-readiness and life skills instruction. Indeed, some sites did not offer these activities at all, while others gave them considerable emphasis. Allentown in Buffalo, for example, spent more per experimental on life skills than it did on education ($1,438, or 25 percent of total average costs).

Expenditures on support service costs ranged from $61 per experimental at CET/San Jose to $1,389 (or 25 percent of the total average cost) at the Los Angeles Job Corps. The other Job Corps sites, as well as SER/Corpus Christi, also devoted a relatively high proportion of resources to these services – about one-fifth to more than one-fourth of total average costs.

Table C.3 shows the breakdown of support services costs for child care, transportation, food, needs-based payments, and other purposes (which included incentives for attendance and achievement). Almost all the sites offered needs-based payments,[19] while about half of them paid for child care and transportation. Food costs were substantial in the three Job Corps sites (ranging from $153 to $259 per experimental), where the on-site cafeterias offered regular meals to both residential and nonresidential participants.

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[19] All Job Corps participants (including those in JOBSTART) received a $40 monthly living allowance, which increased to $60 after they were active for 61 ("good") days, and increased again to $80 after they were active for 181 days. After that, participants were eligible to receive a merit pay level of $90 to $100 per month. In addition, upon termination from the Job Corps, those who remained in the program for a specified length of time received a separate "readjustment allowance" for each month of participation. This allowance ranged from $75 to $100 per month depending on the number of days they were active. Participants could have a portion of this allowance sent to a dependent family member. If they made that choice, the Job Corps contributed an equal amount to the family member.

TABLE C.3

AVERAGE JOBSTART SUPPORT SERVICES AND PARTICIPANT PAYMENT COSTS PER EXPERIMENTAL, BY SITE

	Concurrent							
Support Service	Atlanta Job Corps ($)	CET/ San Jose ($)	Chicago Commons ($)	Connelley (Pittsburgh) ($)	East LA Skills Center ($)	EGOS (Denver) ($)	Phoenix Job Corps ($)	SER/ Corpus Christi ($)
Child care	n/a	n/a	n/a	107	21	339	n/a	86
Transportation	n/a	n/a	n/a	n/a	12	121	n/a	n/a
Food	153	n/a	n/a	n/a	17	7	221	n/a
Needs-based and incentive payments, clothing allowances, and miscellaneous	644	61	363	340	37	0	676	507
Total	797	61	363	447	87	467	897	593

(continued)

TABLE C.3 (continued)

	Sequential/ In-House		Sequential/ Brokered		
Support Service	El Centro (Dallas) ($)	LA Job Corps ($)	Allentown (Buffalo) ($)	BSA (NYC) ($)	CREC (Hartford) ($)
Child care	n/a	n/a	276	45	27
Transportation	161	206	86	107	168
Food	n/a	259	n/a	n/a	n/a
Needs-based and incentive payments, clothing allowances, and miscellaneous	351	924	372	53	37
Total	512	1,389	734	205	232

SOURCE: MDRC calculations from site and MDRC participation, fiscal, and administrative data.

NOTES: Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate.

All costs are in 1986 dollars.

APPENDIX D

SUPPLEMENTAL TABLES FOR CHAPTER 5

TABLE D.1

MONTHLY EARNINGS FOR THE FULL SAMPLE, BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals ($)	Controls ($)	Difference ($)	p^a
Earnings				
Month 1	48	73	-25***	0.003
Month 2	104	156	-52***	0.000
Month 3	118	177	-59***	0.000
Month 4	135	196	-61***	0.000
Month 5	152	217	-65***	0.000
Month 6	164	228	-64***	0.000
Month 7	190	230	-40**	0.013
Month 8	212	235	-23	0.170
Month 9	230	254	-24	0.190
Month 10	240	261	-21	0.249
Month 11	242	283	-41**	0.026
Month 12	263	286	-23	0.207
Month 13	263	265	-2	0.921
Month 14	312	296	16	0.418
Month 15	323	314	9	0.657
Month 16	324	329	-5	0.804
Month 17	323	345	-22	0.279
Month 18	326	343	-17	0.410
Month 19	327	358	-31	0.134
Month 20	341	360	-19	0.395
Month 21	360	367	-7	0.731
Month 22	360	376	-16	0.471
Month 23	363	375	-12	0.597
Month 24	369	384	-15	0.478
Month 25	390	369	21	0.344
Month 26	431	397	34	0.159
Month 27	446	396	50**	0.040
Month 28	448	400	48*	0.055
Month 29	439	397	42*	0.088
Month 30	447	406	41*	0.091
Month 31	451	414	37	0.134
Month 32	449	417	32	0.193
Month 33	441	427	14	0.541
Month 34	446	426	20	0.406
Month 35	465	431	34	0.184
Month 36	477	428	49*	0.057
Month 37	473	418	55**	0.031
Month 38	464	423	41	0.104
Month 39	468	432	36	0.163
Month 40	471	433	38	0.133
Month 41	472	433	39	0.122
Month 42	482	444	38	0.132
Month 43	485	441	44*	0.087
Month 44	475	441	34	0.189
Month 45	458	440	18	0.476
Month 46	460	425	35	0.162
Month 47	450	425	25	0.319
Month 48	436	427	9	0.732
Sample size	988	953		

(continued)

TABLE D.1 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 48 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for differences in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix B). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE D.2

MONTHLY EARNINGS FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals ($)	Controls ($)	Difference ($)	p^a
Earnings				
Month 1	15	26	-11	0.226
Month 2	29	64	-35**	0.019
Month 3	43	75	-32**	0.045
Month 4	64	78	-14	0.460
Month 5	75	81	-6	0.752
Month 6	80	109	-29	0.194
Month 7	94	107	-13	0.563
Month 8	112	113	-1	0.970
Month 9	123	129	-6	0.826
Month 10	122	132	-10	0.706
Month 11	126	130	-4	0.888
Month 12	133	117	16	0.525
Month 13	135	100	35	0.156
Month 14	178	123	55*	0.075
Month 15	183	142	41	0.196
Month 16	178	145	33	0.276
Month 17	180	159	21	0.512
Month 18	181	163	18	0.581
Month 19	181	179	2	0.961
Month 20	191	179	12	0.725
Month 21	178	190	-12	0.751
Month 22	175	196	-21	0.564
Month 23	172	185	-13	0.708
Month 24	164	186	-22	0.523
Month 25	159	194	-35	0.282
Month 26	175	209	-34	0.327
Month 27	201	195	6	0.878
Month 28	215	193	22	0.527
Month 29	220	179	41	0.253
Month 30	229	183	46	0.206
Month 31	239	188	51	0.170
Month 32	246	178	68*	0.072
Month 33	236	187	49	0.205
Month 34	246	203	43	0.282
Month 35	263	227	36	0.378
Month 36	272	236	36	0.386
Month 37	261	234	27	0.503
Month 38	254	235	19	0.635
Month 39	247	235	12	0.767
Month 40	258	225	33	0.414
Month 41	254	222	32	0.426
Month 42	269	228	41	0.326
Month 43	266	246	20	0.632
Month 44	274	251	23	0.587
Month 45	260	243	17	0.696
Month 46	261	225	36	0.375
Month 47	277	242	35	0.405
Month 48	266	270	-4	0.915
Sample size	257	251		

SOURCE AND NOTES: See Table D.1.

TABLE D.3

MONTHLY EARNINGS FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals ($)	Controls ($)	Difference ($)	p^a
Earnings				
Month 1	35	50	-15	0.187
Month 2	86	124	-38*	0.071
Month 3	94	135	-41*	0.057
Month 4	102	151	-49**	0.032
Month 5	122	177	-55**	0.023
Month 6	130	176	-46*	0.068
Month 7	144	191	-47*	0.071
Month 8	163	204	-41	0.126
Month 9	185	182	3	0.920
Month 10	198	190	8	0.771
Month 11	219	227	-8	0.827
Month 12	218	235	-17	0.634
Month 13	220	211	9	0.750
Month 14	266	225	41	0.218
Month 15	270	234	36	0.281
Month 16	265	261	4	0.925
Month 17	259	284	-25	0.467
Month 18	260	297	-37	0.286
Month 19	257	300	-43	0.239
Month 20	262	287	-25	0.488
Month 21	313	298	15	0.685
Month 22	317	282	35	0.378
Month 23	321	291	30	0.445
Month 24	335	299	36	0.350
Month 25	336	271	65	0.101
Month 26	350	310	40	0.341
Month 27	356	330	26	0.551
Month 28	350	339	11	0.796
Month 29	355	347	8	0.854
Month 30	347	338	9	0.835
Month 31	350	351	-1	0.987
Month 32	353	346	7	0.876
Month 33	368	339	29	0.491
Month 34	373	315	58	0.167
Month 35	384	301	83*	0.058
Month 36	386	304	82*	0.066
Month 37	389	304	85*	0.056
Month 38	391	327	64	0.156
Month 39	391	344	47	0.314
Month 40	389	349	40	0.389
Month 41	377	335	42	0.349
Month 42	377	349	28	0.544
Month 43	400	362	38	0.423
Month 44	395	355	40	0.390
Month 45	384	357	27	0.565
Month 46	383	352	31	0.498
Month 47	366	349	17	0.712
Month 48	332	328	4	0.926
Sample size	283	250		

SOURCE AND NOTES: See Table D.1. This subgroup includes women who did not have children.

TABLE D.4

MONTHLY EARNINGS FOR MEN, BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals ($)	Controls ($)	Difference ($)	p^a
Earnings				
Month 1	73	114	-41**	0.015
Month 2	156	228	-72***	0.006
Month 3	174	259	-85***	0.001
Month 4	190	292	-102***	0.000
Month 5	210	319	-109***	0.000
Month 6	228	328	-100***	0.001
Month 7	271	324	-53*	0.066
Month 8	298	320	-22	0.461
Month 9	318	365	-47	0.154
Month 10	330	377	-47	0.147
Month 11	320	403	-83***	0.009
Month 12	361	412	-51	0.117
Month 13	358	391	-33	0.286
Month 14	415	434	-19	0.586
Month 15	433	457	-24	0.481
Month 16	440	472	-32	0.374
Month 17	440	488	-48	0.178
Month 18	446	475	-29	0.402
Month 19	450	495	-45	0.204
Month 20	474	503	-29	0.419
Month 21	492	506	-14	0.683
Month 22	490	532	-42	0.244
Month 23	495	532	-37	0.311
Month 24	504	546	-42	0.258
Month 25	553	524	29	0.468
Month 26	625	553	72*	0.091
Month 27	638	548	90**	0.033
Month 28	637	556	81*	0.062
Month 29	609	554	55	0.199
Month 30	626	577	49	0.260
Month 31	627	584	43	0.319
Month 32	618	596	22	0.609
Month 33	599	615	-16	0.708
Month 34	600	619	-19	0.660
Month 35	625	622	3	0.951
Month 36	645	609	36	0.414
Month 37	643	586	57	0.184
Month 38	625	585	40	0.348
Month 39	641	593	48	0.264
Month 40	646	594	52	0.221
Month 41	657	606	51	0.225
Month 42	666	620	46	0.274
Month 43	657	600	57	0.183
Month 44	635	599	36	0.396
Month 45	616	597	19	0.651
Month 46	620	580	40	0.338
Month 47	600	572	28	0.508
Month 48	593	575	18	0.664
Sample size	988	953		

SOURCE AND NOTES: See Table D.1.

TABLE D.5

MONTHLY EMPLOYMENT RATES FOR THE FULL SAMPLE, BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	p^a
Ever employed				
Month 1	13.7	20.4	-6.7***	0.000
Month 2	16.6	24.9	-8.4***	0.000
Month 3	19.0	27.8	-8.8***	0.000
Month 4	21.6	30.7	-9.2***	0.000
Month 5	24.0	33.1	-9.1***	0.000
Month 6	26.6	33.7	-7.1***	0.000
Month 7	30.8	34.5	-3.8*	0.064
Month 8	32.9	35.5	-2.6	0.208
Month 9	34.8	36.7	-1.9	0.367
Month 10	37.4	38.3	-0.9	0.657
Month 11	37.3	40.6	-3.3	0.121
Month 12	40.4	42.0	-1.6	0.435
Month 13	47.0	44.0	3.0	0.162
Month 14	40.2	36.9	3.3	0.111
Month 15	41.9	39.3	2.6	0.216
Month 16	42.0	40.0	1.9	0.357
Month 17	41.4	42.4	-1.0	0.634
Month 18	41.9	43.0	-1.1	0.600
Month 19	41.5	43.7	-2.2	0.298
Month 20	42.3	43.4	-1.1	0.598
Month 21	44.2	43.6	0.6	0.778
Month 22	45.1	44.1	1.1	0.616
Month 23	44.6	44.9	-0.3	0.902
Month 24	45.3	44.9	0.4	0.837
Month 25	42.7	42.4	0.4	0.858
Month 26	45.3	42.8	2.5	0.235
Month 27	46.0	43.5	2.6	0.219
Month 28	46.5	43.1	3.3	0.118
Month 29	46.0	42.9	3.1	0.142
Month 30	46.8	44.5	2.3	0.287
Month 31	46.9	45.7	1.2	0.560
Month 32	47.1	45.4	1.8	0.407
Month 33	46.8	45.4	1.4	0.515
Month 34	46.5	46.2	0.3	0.873
Month 35	48.4	46.1	2.3	0.278
Month 36	48.1	45.0	3.1	0.146
Month 37	49.2	45.6	3.5*	0.097
Month 38	47.7	46.1	1.6	0.465
Month 39	48.2	46.6	1.6	0.449
Month 40	48.4	46.9	1.6	0.466
Month 41	48.0	46.9	1.1	0.598
Month 42	48.7	46.0	2.7	0.200
Month 43	48.9	47.2	1.8	0.403
Month 44	48.8	47.0	1.8	0.413
Month 45	47.4	47.4	0.0	0.984
Month 46	47.8	46.0	1.8	0.397
Month 47	47.5	45.6	1.9	0.373
Month 48	45.4	46.2	-0.9	0.682
Sample size	988	953		

SOURCE AND NOTES: See Table D.1.

TABLE D.6

MONTHLY HOURS WORKED FOR THE FULL SAMPLE, BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Total hours worked				
Month 1	9.96	15.68	-5.72***	0.000
Month 2	22.15	34.16	-12.01***	0.000
Month 3	25.71	38.66	-12.95***	0.000
Month 4	29.06	42.89	-13.83***	0.000
Month 5	32.41	47.64	-15.23***	0.000
Month 6	34.93	48.69	-13.76***	0.000
Month 7	40.16	48.47	-8.31***	0.008
Month 8	44.40	49.95	-5.55*	0.089
Month 9	47.64	52.94	-5.30	0.114
Month 10	49.63	54.29	-4.66	0.163
Month 11	50.51	58.15	-7.64**	0.023
Month 12	54.53	58.60	-4.08	0.221
Month 13	52.19	51.63	0.57	0.855
Month 14	59.99	56.35	3.64	0.297
Month 15	62.00	59.70	2.30	0.513
Month 16	61.91	62.37	-0.46	0.895
Month 17	61.75	65.63	-3.88	0.279
Month 18	62.73	65.37	-2.64	0.458
Month 19	62.80	67.82	-5.02	0.164
Month 20	64.43	67.75	-3.32	0.364
Month 21	67.65	68.58	-0.93	0.800
Month 22	67.57	69.65	-2.09	0.567
Month 23	68.04	69.46	-1.42	0.697
Month 24	69.17	70.98	-1.81	0.621
Month 25	65.71	65.56	0.14	0.967
Month 26	72.96	69.94	3.02	0.414
Month 27	75.01	70.01	5.00	0.179
Month 28	74.53	70.19	4.34	0.246
Month 29	73.50	69.79	3.71	0.323
Month 30	75.45	71.09	4.36	0.243
Month 31	76.24	72.65	3.58	0.336
Month 32	76.41	72.34	4.07	0.278
Month 33	75.66	73.61	2.05	0.584
Month 34	75.77	73.17	2.59	0.485
Month 35	78.03	73.55	4.49	0.230
Month 36	79.68	72.91	6.77*	0.074
Month 37	79.07	72.06	7.00*	0.065
Month 38	77.18	72.80	4.38	0.248
Month 39	77.71	74.63	3.08	0.419
Month 40	78.10	74.41	3.69	0.329
Month 41	78.05	74.41	3.64	0.333
Month 42	79.72	75.60	4.12	0.282
Month 43	79.66	75.44	4.23	0.270
Month 44	78.74	76.02	2.73	0.476
Month 45	76.67	75.89	0.78	0.837
Month 46	76.99	72.62	4.37	0.245
Month 47	75.39	73.09	2.29	0.545
Month 48	73.01	73.44	-0.43	0.908
Sample size	988	953		

SOURCE AND NOTES: See Table D.1.

TABLE D.7

MONTHLY WEEKS WORKED FOR THE FULL SAMPLE, BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Number of weeks employed				
Month 1	0.30	0.44	-0.14***	0.001
Month 2	0.65	0.96	-0.30***	0.000
Month 3	0.74	1.09	-0.35***	0.000
Month 4	0.85	1.20	-0.35***	0.000
Month 5	0.94	1.32	-0.37***	0.000
Month 6	1.01	1.34	-0.34***	0.000
Month 7	1.15	1.36	-0.20**	0.013
Month 8	1.27	1.40	-0.13	0.133
Month 9	1.35	1.46	-0.11	0.211
Month 10	1.41	1.52	-0.11	0.201
Month 11	1.44	1.60	-0.16*	0.061
Month 12	1.55	1.62	-0.07	0.428
Month 13	1.45	1.40	0.04	0.594
Month 14	1.62	1.48	0.14	0.106
Month 15	1.68	1.56	0.12	0.170
Month 16	1.68	1.63	0.05	0.555
Month 17	1.67	1.71	-0.04	0.622
Month 18	1.68	1.73	-0.05	0.611
Month 19	1.68	1.78	-0.10	0.271
Month 20	1.69	1.76	-0.08	0.398
Month 21	1.79	1.78	0.01	0.913
Month 22	1.79	1.80	-0.01	0.871
Month 23	1.79	1.80	-0.02	0.858
Month 24	1.82	1.84	-0.02	0.811
Month 25	1.77	1.77	0.00	0.997
Month 26	1.87	1.81	0.07	0.441
Month 27	1.93	1.81	0.12	0.182
Month 28	1.92	1.81	0.11	0.226
Month 29	1.90	1.81	0.09	0.323
Month 30	1.95	1.85	0.10	0.294
Month 31	1.98	1.89	0.08	0.372
Month 32	1.97	1.88	0.08	0.357
Month 33	1.95	1.91	0.04	0.680
Month 34	1.95	1.91	0.05	0.617
Month 35	1.98	1.91	0.07	0.454
Month 36	2.02	1.90	0.12	0.196
Month 37	2.00	1.88	0.13	0.163
Month 38	1.97	1.91	0.06	0.509
Month 39	2.00	1.94	0.05	0.571
Month 40	2.00	1.94	0.06	0.494
Month 41	1.99	1.92	0.07	0.464
Month 42	2.03	1.92	0.11	0.238
Month 43	2.03	1.92	0.10	0.264
Month 44	2.01	1.94	0.07	0.417
Month 45	1.97	1.93	0.04	0.694
Month 46	1.98	1.87	0.11	0.236
Month 47	1.93	1.88	0.05	0.585
Month 48	1.87	1.89	-0.02	0.809
Sample size	988	953		

SOURCE AND NOTES: See Table D.1.

TABLE D.8

MONTHLY EMPLOYMENT RATES FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	p[a]
Ever employed				
Month 1	6.2	8.7	-2.5	0.290
Month 2	6.4	12.2	-5.8**	0.025
Month 3	9.9	14.1	-4.2	0.153
Month 4	12.4	15.5	-3.1	0.321
Month 5	14.7	16.4	-1.7	0.611
Month 6	16.6	18.1	-1.5	0.670
Month 7	19.1	19.1	-0.1	0.986
Month 8	21.6	21.4	0.2	0.957
Month 9	23.6	23.3	0.3	0.929
Month 10	25.5	23.3	2.3	0.554
Month 11	23.6	23.3	0.3	0.943
Month 12	24.3	23.8	0.5	0.894
Month 13	30.2	23.2	7.0*	0.080
Month 14	26.5	19.5	7.1*	0.063
Month 15	26.8	22.8	4.0	0.300
Month 16	27.6	21.6	6.0	0.113
Month 17	26.5	23.9	2.6	0.502
Month 18	26.5	24.7	1.9	0.637
Month 19	26.3	26.1	0.1	0.971
Month 20	24.7	26.9	-2.1	0.588
Month 21	23.4	27.4	-4.0	0.303
Month 22	23.6	26.9	-3.3	0.396
Month 23	24.0	25.2	-1.2	0.760
Month 24	23.2	23.7	-0.5	0.897
Month 25	20.0	27.0	-7.0*	0.060
Month 26	21.8	27.5	-5.7	0.132
Month 27	24.2	26.2	-2.1	0.590
Month 28	26.0	25.6	0.4	0.918
Month 29	27.2	24.0	3.2	0.411
Month 30	27.2	24.7	2.5	0.520
Month 31	29.5	25.2	4.3	0.281
Month 32	28.8	24.7	4.0	0.311
Month 33	27.5	23.7	3.8	0.335
Month 34	27.2	26.3	0.8	0.832
Month 35	29.3	29.0	0.3	0.943
Month 36	32.0	27.4	4.5	0.263
Month 37	32.0	28.6	3.3	0.417
Month 38	29.5	30.3	-0.8	0.850
Month 39	29.7	29.8	-0.1	0.983
Month 40	31.5	27.9	3.7	0.364
Month 41	30.1	27.8	2.3	0.558
Month 42	31.1	27.5	3.6	0.370
Month 43	30.2	29.7	0.5	0.899
Month 44	31.6	29.4	2.1	0.607
Month 45	29.9	29.2	0.7	0.857
Month 46	31.2	25.8	5.4	0.178
Month 47	33.5	29.0	4.5	0.275
Month 48	32.2	30.4	1.8	0.656
Sample size	988	953		

SOURCE AND NOTES: See Table D.1.

TABLE D.9

MONTHLY HOURS WORKED FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Total hours worked				
Month 1	3.83	6.54	-2.71	0.188
Month 2	7.76	15.72	-7.96**	0.029
Month 3	11.26	18.75	-7.48*	0.064
Month 4	15.56	19.66	-4.10	0.351
Month 5	17.56	20.31	-2.75	0.542
Month 6	18.62	25.28	-6.65	0.180
Month 7	22.43	24.94	-2.51	0.617
Month 8	27.54	27.29	0.25	0.964
Month 9	30.17	30.53	-0.36	0.949
Month 10	29.99	30.69	-0.70	0.901
Month 11	30.31	30.81	-0.50	0.930
Month 12	31.24	28.46	2.78	0.617
Month 13	29.38	22.16	7.22	0.151
Month 14	36.31	26.31	10.00*	0.082
Month 15	36.59	30.54	6.05	0.301
Month 16	35.46	31.08	4.38	0.449
Month 17	35.65	33.51	2.14	0.724
Month 18	36.55	34.64	1.91	0.753
Month 19	36.41	36.87	-0.45	0.941
Month 20	36.33	37.08	-0.74	0.906
Month 21	33.00	38.34	-5.34	0.389
Month 22	31.60	38.02	-6.42	0.283
Month 23	30.95	35.42	-4.47	0.449
Month 24	29.85	37.18	-7.33	0.227
Month 25	30.79	38.89	-8.10	0.166
Month 26	34.49	41.80	-7.31	0.235
Month 27	38.93	39.08	-0.15	0.981
Month 28	40.80	38.35	2.45	0.699
Month 29	41.71	35.54	6.17	0.330
Month 30	43.69	36.57	7.12	0.267
Month 31	45.27	37.31	7.96	0.215
Month 32	45.80	35.09	10.71*	0.098
Month 33	43.60	34.90	8.70	0.181
Month 34	44.34	36.51	7.83	0.227
Month 35	47.47	40.51	6.96	0.288
Month 36	49.07	42.00	7.07	0.286
Month 37	48.02	41.92	6.11	0.351
Month 38	44.82	41.69	3.13	0.631
Month 39	44.09	42.23	1.86	0.772
Month 40	45.40	40.28	5.12	0.416
Month 41	45.08	40.28	4.80	0.452
Month 42	46.28	40.44	5.84	0.372
Month 43	44.23	42.30	1.93	0.764
Month 44	46.67	43.08	3.59	0.586
Month 45	44.79	42.13	2.66	0.683
Month 46	44.71	37.85	6.86	0.289
Month 47	47.80	41.07	6.74	0.307
Month 48	46.57	45.61	0.96	0.886
Sample size	257	251		

SOURCE AND NOTES: See Table D.1.

TABLE D.10

MONTHLY WEEKS WORKED FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Number of weeks employed				
Month 1	0.13	0.17	-0.05	0.423
Month 2	0.27	0.46	-0.19*	0.073
Month 3	0.35	0.56	-0.20*	0.077
Month 4	0.47	0.58	-0.11	0.376
Month 5	0.57	0.61	-0.04	0.751
Month 6	0.60	0.72	-0.13	0.355
Month 7	0.70	0.73	-0.04	0.797
Month 8	0.84	0.80	0.04	0.774
Month 9	0.88	0.92	-0.04	0.771
Month 10	0.89	0.91	-0.02	0.880
Month 11	0.89	0.91	-0.02	0.898
Month 12	0.90	0.88	0.02	0.885
Month 13	0.83	0.66	0.17	0.190
Month 14	1.04	0.77	0.27*	0.079
Month 15	1.05	0.87	0.18	0.255
Month 16	1.05	0.88	0.17	0.285
Month 17	1.03	0.93	0.10	0.519
Month 18	1.04	0.99	0.06	0.719
Month 19	1.03	1.04	-0.01	0.951
Month 20	1.01	1.05	-0.04	0.813
Month 21	0.93	1.07	-0.14	0.379
Month 22	0.93	1.07	-0.14	0.372
Month 23	0.92	0.98	-0.06	0.682
Month 24	0.87	0.97	-0.10	0.541
Month 25	0.82	1.13	-0.32**	0.042
Month 26	0.89	1.14	-0.25	0.118
Month 27	1.02	1.08	-0.06	0.690
Month 28	1.06	1.08	-0.02	0.911
Month 29	1.08	1.00	0.08	0.640
Month 30	1.15	1.03	0.13	0.454
Month 31	1.20	1.05	0.15	0.369
Month 32	1.19	0.99	0.20	0.225
Month 33	1.14	0.98	0.16	0.347
Month 34	1.13	1.05	0.08	0.635
Month 35	1.22	1.13	0.08	0.617
Month 36	1.27	1.18	0.09	0.585
Month 37	1.26	1.17	0.09	0.599
Month 38	1.18	1.20	-0.01	0.940
Month 39	1.21	1.21	0.00	0.980
Month 40	1.26	1.15	0.11	0.517
Month 41	1.23	1.14	0.09	0.578
Month 42	1.26	1.13	0.13	0.435
Month 43	1.24	1.18	0.06	0.715
Month 44	1.29	1.19	0.10	0.562
Month 45	1.25	1.13	0.12	0.464
Month 46	1.26	1.03	0.23	0.168
Month 47	1.34	1.15	0.19	0.272
Month 48	1.30	1.24	0.06	0.738
Sample size	257	251		

SOURCE AND NOTES: See Table D.1.

TABLE D.11

MONTHLY EMPLOYMENT RATES FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	p^a
Ever employed				
Month 1	13.3	18.5	-5.2*	0.092
Month 2	16.8	22.1	-5.3	0.113
Month 3	17.8	24.2	-6.4*	0.060
Month 4	18.5	26.7	-8.2**	0.020
Month 5	22.3	31.1	-8.8**	0.018
Month 6	24.2	31.8	-7.6**	0.045
Month 7	29.0	32.8	-3.8	0.341
Month 8	31.1	34.4	-3.3	0.421
Month 9	33.4	30.2	3.3	0.414
Month 10	34.8	31.0	3.8	0.353
Month 11	36.5	33.9	2.6	0.530
Month 12	38.1	38.1	0.0	0.995
Month 13	43.7	40.1	3.6	0.386
Month 14	37.3	31.8	5.6	0.163
Month 15	38.2	33.5	4.7	0.248
Month 16	38.2	36.8	1.4	0.726
Month 17	37.3	39.0	-1.6	0.689
Month 18	37.0	38.9	-1.9	0.638
Month 19	36.7	39.3	-2.6	0.542
Month 20	36.3	36.1	0.2	0.956
Month 21	43.8	35.2	8.6**	0.043
Month 22	43.4	32.8	10.6**	0.012
Month 23	43.3	36.6	6.7	0.117
Month 24	42.5	37.5	5.0	0.238
Month 25	40.1	32.6	7.4*	0.078
Month 26	41.2	32.5	8.7**	0.038
Month 27	40.4	35.9	4.5	0.283
Month 28	41.4	36.4	5.0	0.240
Month 29	42.4	36.8	5.6	0.190
Month 30	41.7	37.2	4.5	0.288
Month 31	41.5	40.6	0.9	0.831
Month 32	41.7	40.0	1.7	0.697
Month 33	43.7	39.3	4.4	0.297
Month 34	43.7	37.3	6.3	0.133
Month 35	44.3	35.8	8.5**	0.045
Month 36	43.1	34.8	8.4**	0.049
Month 37	44.5	35.7	8.8**	0.038
Month 38	44.9	38.3	6.6	0.128
Month 39	43.1	39.2	4.0	0.359
Month 40	44.0	40.9	3.1	0.470
Month 41	42.1	39.5	2.5	0.546
Month 42	40.5	38.1	2.4	0.559
Month 43	43.6	39.9	3.7	0.381
Month 44	44.1	40.5	3.6	0.395
Month 45	42.7	41.2	1.5	0.729
Month 46	43.3	39.7	3.6	0.398
Month 47	42.8	38.4	4.4	0.298
Month 48	37.9	37.9	0.0	0.993
Sample size	283	250		

SOURCE AND NOTES: See Table D.1. This subgroup includes women who did not have children.

TABLE D.12

MONTHLY HOURS WORKED FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Total hours worked				
Month 1	8.71	11.69	-2.98	0.253
Month 2	20.93	29.07	-8.14*	0.083
Month 3	23.12	31.65	-8.52*	0.089
Month 4	24.40	34.70	-10.31**	0.044
Month 5	29.23	40.76	-11.53**	0.032
Month 6	31.15	40.39	-9.23*	0.091
Month 7	34.96	43.85	-8.90	0.119
Month 8	38.48	47.31	-8.83	0.138
Month 9	42.06	41.76	0.31	0.959
Month 10	44.21	42.59	1.62	0.787
Month 11	48.68	48.46	0.21	0.973
Month 12	47.42	49.07	-1.64	0.784
Month 13	46.22	42.92	3.30	0.545
Month 14	54.08	44.83	9.24	0.141
Month 15	54.55	47.16	7.39	0.245
Month 16	53.67	53.49	0.18	0.978
Month 17	52.80	57.56	-4.76	0.464
Month 18	53.60	59.33	-5.73	0.385
Month 19	53.12	58.82	-5.70	0.396
Month 20	53.41	56.36	-2.95	0.665
Month 21	63.56	56.63	6.93	0.327
Month 22	64.11	52.75	11.36	0.109
Month 23	63.95	54.84	9.10	0.195
Month 24	64.96	56.58	8.38	0.235
Month 25	59.17	49.00	10.16	0.129
Month 26	62.41	54.10	8.31	0.242
Month 27	63.27	58.97	4.30	0.551
Month 28	61.95	60.24	1.70	0.814
Month 29	62.80	62.48	0.32	0.965
Month 30	60.91	60.77	0.14	0.984
Month 31	62.04	63.39	-1.35	0.850
Month 32	63.14	62.61	0.52	0.941
Month 33	65.86	61.51	4.35	0.539
Month 34	65.19	57.56	7.63	0.272
Month 35	66.12	56.02	10.10	0.154
Month 36	67.11	55.39	11.72	0.104
Month 37	68.00	56.18	11.82	0.101
Month 38	68.43	61.52	6.91	0.349
Month 39	68.41	64.19	4.22	0.578
Month 40	67.69	64.49	3.20	0.667
Month 41	64.47	61.06	3.41	0.638
Month 42	63.98	62.98	1.00	0.892
Month 43	68.04	65.05	2.99	0.687
Month 44	67.96	64.33	3.63	0.624
Month 45	66.84	64.15	2.69	0.713
Month 46	67.81	61.43	6.38	0.375
Month 47	65.71	62.78	2.93	0.687
Month 48	59.49	59.44	0.06	0.994
Sample size	283	250		

SOURCE AND NOTES: See Table D.1. This subgroup includes women who did not have children.

TABLE D.13

MONTHLY WEEKS WORKED FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Number of weeks employed				
Month 1	0.28	0.38	-0.10	0.212
Month 2	0.66	0.87	-0.21	0.116
Month 3	0.69	0.93	-0.24*	0.086
Month 4	0.71	1.01	-0.30**	0.036
Month 5	0.87	1.20	-0.34**	0.026
Month 6	0.93	1.19	-0.26*	0.084
Month 7	1.03	1.28	-0.24	0.121
Month 8	1.18	1.34	-0.15	0.350
Month 9	1.23	1.18	0.05	0.758
Month 10	1.30	1.23	0.06	0.704
Month 11	1.44	1.34	0.10	0.545
Month 12	1.42	1.39	0.02	0.882
Month 13	1.33	1.23	0.10	0.500
Month 14	1.50	1.22	0.28*	0.093
Month 15	1.54	1.30	0.24	0.152
Month 16	1.51	1.46	0.05	0.771
Month 17	1.49	1.59	-0.10	0.557
Month 18	1.50	1.59	-0.09	0.588
Month 19	1.49	1.57	-0.09	0.627
Month 20	1.45	1.47	-0.02	0.900
Month 21	1.72	1.46	0.26	0.142
Month 22	1.71	1.36	0.35**	0.047
Month 23	1.70	1.46	0.25	0.158
Month 24	1.72	1.52	0.20	0.264
Month 25	1.66	1.32	0.34*	0.050
Month 26	1.72	1.37	0.35**	0.049
Month 27	1.74	1.51	0.24	0.188
Month 28	1.70	1.51	0.19	0.299
Month 29	1.75	1.56	0.19	0.307
Month 30	1.71	1.54	0.18	0.331
Month 31	1.74	1.65	0.09	0.616
Month 32	1.75	1.66	0.09	0.621
Month 33	1.81	1.63	0.18	0.321
Month 34	1.82	1.53	0.29	0.101
Month 35	1.83	1.47	0.35*	0.050
Month 36	1.82	1.47	0.35*	0.053
Month 37	1.82	1.47	0.35**	0.049
Month 38	1.86	1.60	0.26	0.147
Month 39	1.85	1.65	0.19	0.300
Month 40	1.81	1.65	0.15	0.399
Month 41	1.71	1.55	0.15	0.378
Month 42	1.69	1.58	0.11	0.535
Month 43	1.80	1.66	0.14	0.440
Month 44	1.81	1.63	0.18	0.303
Month 45	1.79	1.63	0.15	0.401
Month 46	1.80	1.57	0.23	0.198
Month 47	1.71	1.61	0.10	0.568
Month 48	1.55	1.54	0.01	0.950
Sample size	283	250		

SOURCE AND NOTES: See Table D.1. This subgroup includes women who did not have children.

TABLE D.14

MONTHLY EMPLOYMENT RATES FOR MEN, BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	p^a
Ever employed				
Month 1	17.8	28.1	-10.3***	0.000
Month 2	21.5	34.2	-12.7***	0.000
Month 3	24.3	37.9	-13.7***	0.000
Month 4	27.6	42.5	-14.9***	0.000
Month 5	29.3	44.4	-15.2***	0.000
Month 6	32.8	44.5	-11.7***	0.000
Month 7	38.2	44.5	-6.3*	0.051
Month 8	40.5	44.1	-3.6	0.267
Month 9	41.8	47.9	-6.1*	0.065
Month 10	45.3	51.1	-5.8*	0.075
Month 11	45.7	54.0	-8.4**	0.011
Month 12	51.0	54.3	-3.4	0.299
Month 13	58.3	57.9	0.4	0.902
Month 14	49.5	49.9	-0.4	0.907
Month 15	52.9	51.7	1.2	0.717
Month 16	52.2	52.4	-0.2	0.957
Month 17	51.9	55.2	-3.2	0.321
Month 18	53.4	55.9	-2.5	0.442
Month 19	53.0	56.3	-3.3	0.308
Month 20	55.9	57.0	-1.1	0.745
Month 21	56.4	57.4	-1.0	0.755
Month 22	58.3	60.1	-1.8	0.585
Month 23	57.1	60.7	-3.6	0.273
Month 24	59.4	61.2	-1.8	0.578
Month 25	57.4	56.4	1.0	0.763
Month 26	61.2	57.0	4.2	0.198
Month 27	62.0	57.3	4.7	0.144
Month 28	61.0	57.0	4.1	0.210
Month 29	58.6	57.2	1.3	0.683
Month 30	60.3	60.3	0.0	0.993
Month 31	59.8	60.4	-0.6	0.852
Month 32	60.5	60.4	0.0	0.988
Month 33	59.6	61.1	-1.5	0.640
Month 34	59.3	62.3	-3.0	0.350
Month 35	61.7	61.4	0.3	0.937
Month 36	60.4	60.7	-0.4	0.904
Month 37	61.9	60.7	1.2	0.710
Month 38	59.4	59.5	-0.2	0.957
Month 39	62.0	60.3	1.7	0.588
Month 40	61.1	60.5	0.6	0.860
Month 41	62.1	61.5	0.6	0.842
Month 42	63.7	60.8	2.9	0.360
Month 43	62.6	61.4	1.2	0.703
Month 44	61.3	60.7	0.5	0.873
Month 45	60.1	61.2	-1.1	0.728
Month 46	60.0	60.7	-0.7	0.820
Month 47	58.2	58.9	-0.7	0.826
Month 48	57.3	60.0	-2.8	0.383
Sample size	448	452		

SOURCE AND NOTES: See Table D.1.

TABLE D.15

MONTHLY HOURS WORKED FOR MEN, BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Total hours worked				
Month 1	13.76	23.47	-9.71***	0.001
Month 2	30.36	48.01	-17.65***	0.000
Month 3	34.85	54.37	-19.52***	0.000
Month 4	38.31	61.73	-23.42***	0.000
Month 5	41.45	68.10	-26.65***	0.000
Month 6	45.23	67.71	-22.48***	0.000
Month 7	52.65	65.07	-12.42**	0.018
Month 8	57.21	64.60	-7.39	0.172
Month 9	60.55	72.20	-11.65**	0.037
Month 10	63.38	74.80	-11.42**	0.039
Month 11	62.63	79.32	-16.68***	0.002
Month 12	71.52	81.46	-9.94*	0.073
Month 13	67.97	73.88	-5.91	0.261
Month 14	76.66	80.05	-3.38	0.561
Month 15	80.46	83.63	-3.17	0.588
Month 16	81.45	85.50	-4.05	0.489
Month 17	81.33	88.96	-7.63	0.197
Month 18	82.31	86.98	-4.67	0.416
Month 19	82.91	91.11	-8.20	0.162
Month 20	86.71	91.88	-5.18	0.380
Month 21	89.65	92.46	-2.81	0.630
Month 22	89.61	97.33	-7.72	0.186
Month 23	90.92	97.42	-6.50	0.273
Month 24	93.84	98.27	-4.43	0.451
Month 25	89.46	89.95	-0.49	0.931
Month 26	101.40	94.63	6.77	0.257
Month 27	102.75	93.68	9.07	0.128
Month 28	101.13	94.08	7.06	0.240
Month 29	97.41	93.92	3.49	0.561
Month 30	101.48	97.32	4.16	0.488
Month 31	101.72	98.65	3.06	0.609
Month 32	101.32	99.44	1.88	0.754
Month 33	99.54	102.51	-2.97	0.621
Month 34	99.83	102.80	-2.97	0.620
Month 35	102.66	102.00	0.66	0.911
Month 36	104.65	100.31	4.34	0.474
Month 37	103.76	97.71	6.05	0.321
Month 38	100.63	96.95	3.68	0.542
Month 39	102.63	98.63	4.00	0.509
Month 40	103.65	98.65	5.00	0.408
Month 41	105.50	100.79	4.71	0.430
Month 42	108.14	102.81	5.33	0.381
Month 43	105.96	100.95	5.01	0.414
Month 44	102.95	101.76	1.20	0.844
Month 45	100.48	101.79	-1.31	0.829
Month 46	100.57	98.85	1.71	0.776
Month 47	96.78	97.13	-0.34	0.955
Month 48	95.93	97.42	-1.49	0.802
Sample size	448	452		

SOURCE AND NOTES: See Table D.1.

TABLE D.16

MONTHLY WEEKS WORKED FOR MEN, BY RESEARCH STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Number of weeks employed				
Month 1	0.39	0.64	-0.24***	0.001
Month 2	0.85	1.31	-0.46***	0.000
Month 3	0.97	1.50	-0.52***	0.000
Month 4	1.10	1.68	-0.58***	0.000
Month 5	1.17	1.82	-0.65***	0.000
Month 6	1.25	1.81	-0.56***	0.000
Month 7	1.46	1.77	-0.31**	0.019
Month 8	1.56	1.78	-0.22	0.112
Month 9	1.69	1.93	-0.23*	0.092
Month 10	1.77	2.03	-0.27*	0.052
Month 11	1.74	2.13	-0.39***	0.004
Month 12	2.00	2.17	-0.17	0.202
Month 13	1.84	1.94	-0.10	0.453
Month 14	2.01	2.03	-0.02	0.893
Month 15	2.12	2.11	0.01	0.941
Month 16	2.13	2.15	-0.02	0.881
Month 17	2.12	2.23	-0.11	0.422
Month 18	2.14	2.25	-0.10	0.448
Month 19	2.15	2.32	-0.18	0.209
Month 20	2.22	2.34	-0.12	0.369
Month 21	2.31	2.35	-0.04	0.763
Month 22	2.31	2.47	-0.16	0.259
Month 23	2.32	2.48	-0.16	0.248
Month 24	2.41	2.51	-0.10	0.458
Month 25	2.39	2.38	0.01	0.951
Month 26	2.53	2.42	0.11	0.425
Month 27	2.57	2.39	0.18	0.200
Month 28	2.54	2.40	0.14	0.311
Month 29	2.44	2.42	0.02	0.891
Month 30	2.52	2.51	0.01	0.967
Month 31	2.54	2.52	0.02	0.892
Month 32	2.53	2.53	0.00	0.983
Month 33	2.49	2.60	-0.11	0.419
Month 34	2.50	2.60	-0.11	0.446
Month 35	2.51	2.60	-0.09	0.516
Month 36	2.56	2.56	0.01	0.959
Month 37	2.54	2.50	0.04	0.771
Month 38	2.47	2.50	-0.03	0.811
Month 39	2.54	2.52	0.02	0.873
Month 40	2.56	2.53	0.03	0.822
Month 41	2.61	2.56	0.05	0.736
Month 42	2.67	2.55	0.12	0.396
Month 43	2.59	2.51	0.08	0.545
Month 44	2.53	2.54	0.00	0.972
Month 45	2.48	2.55	-0.07	0.616
Month 46	2.50	2.52	-0.02	0.892
Month 47	2.41	2.45	-0.05	0.726
Month 48	2.37	2.46	-0.09	0.519
Sample size	448	452		

SOURCE AND NOTES: See Table D.1.

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